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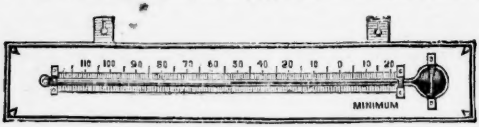

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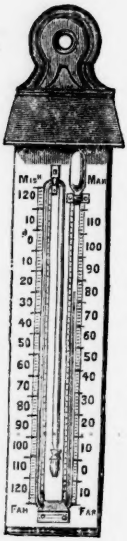

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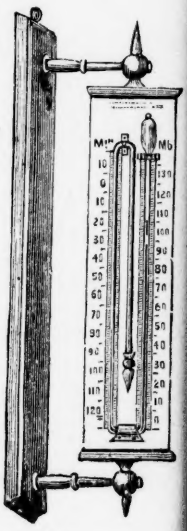

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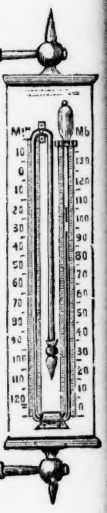

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THE

JOURNAL OF SCIENCE.

MARCH, 1880.

I. THE LINES OF DISCOVERY IN THE HISTORY OF OZONE.

By ALBERT R. LEEDS, Ph.D.

I. *Its Original Discovery, Sources, and Properties.*

THE history of ozone begins with the clear apprehension, in the year 1840, by Schönbein, that in the odour given off in the electrolysis of water, and accompanying discharges of frictional electricity in air, he had to deal with a distinct and important phenomenon. Schönbein's discovery did not consist in noting the odour; that had been done by Van Marum, more than half a century before, but in first appreciating the importance and true meaning of the phenomenon. For while Van Marum, Cavallo, and others who followed them, connected the odour with the electricity, calling it the "electrical odour" or "aura electrica," and thus made it the property of an imponderable agent, Schönbein ascribed it to the peculiar form of matter operated upon. The hypothesis of Van Marum necessarily remained barren of fruits; that of Schönbein speedily enriched chemical science with a host of acquisitions. Clinging tenaciously to the doctrine that there could not be a variety of origin for one and the same odour, and that the kind of matter producing it in every case must be identical, Schönbein fixed his discovery by giving to that one and certain kind of matter the name of Ozone. By adhering to this guiding clue, he added, as a third source of ozone, the action of moist phosphorus upon air (1840 to 1843), and since that time, besides electrolysis, electrical influence, and

the action of air upon moist phosphorus, no other sources of ozone of practical utility have been discovered.

The fact that Schönbein so stoutly insisted on, and eventually so triumphantly established, the *identity* of the ozone from whatever source derived, must not be lost sight of in any estimate of his merits as a discoverer. The earliest attack came from De la Rive, who attributed the odour to metallic oxides set free from the metals used as electrodes, or as terminals in electric discharges. But Schönbein pointed out that besides the improbability of an odour arising from solid bodies, this hypothesis required that solid bodies should have the property of indefinite suspension in the atmosphere instead of being deposited or washed down by water (1840 to 1843).

The next attacks came from Fischer, who regarded Schönbein's ozone as probably peroxide of hydrogen, and from Williamson, who thought there were two kinds of ozone,—one the ozone given off in electrolysis, and which he regarded as a higher oxide of hydrogen, differing from the previously well-known peroxide, and the other formed by the action of phosphorus on moist air. But Schönbein disposed of both objections: the first, by showing that the chemical and physical properties of ozone are not the properties of peroxide of hydrogen; the second, by demonstrating that whatever might be the true nature of ozone, the gaseous matter obtained in the electrolysis of water was in all respects identical with that formed by the action upon air of moist phosphorus (1844 to 1845).

During these first five years Schönbein was busily engaged in ascertaining the properties of ozone. Since no peculiar methods were employed in the furtherance of these discoveries, they need not detain us here further than briefly to summarise them, and to point out what corrections have been rendered necessary by the labours of subsequent investigators. They are—1st. Its eminent oxidising powers, as shown by its ability to transform most metals into their higher oxides, and to raise the lower oxides into the condition of peroxides. Certain of the non-metals—phosphorus, chlorine, bromine, and iodine—are similarly oxidised. Schönbein's statement that it does not unite with nitrogen under ordinary circumstances, but enters into combination when alkali is present, has been abundantly disproved—among others, by Berthelot (1878), who has shown that no combination occurs even when alkali is present. It oxidises sulphites and nitrites into sulphates and nitrates, and many sulphides into their corresponding sulphates. It destroys

(as has since been more elaborately demonstrated by Houzeau, 1872) many gaseous compounds of hydrogen, like those with sulphur, selenium, phosphorus, iodine, arsenic, and antimony. It discharges vegetable colours, and powerfully attacks many organic bodies. The nature of its action in the latter case has been more extensively studied by Gorup-Besanez (1863), and he has described the products of the reactions which occur when ozone is allowed to act upon organic substances, alone or in presence of alkali. 2nd. According to Schönbein ozone is insoluble in water. The observations of subsequent experimenters conflict on this point, but there appears to be much evidence to show that it is soluble in water, though only in small degree. 3rd. Schönbein pointed out that atmospheric air strongly charged with ozone acts powerfully on the mucous membranes and produces symptoms of catarrh. This and his analogous discovery that ozone is present in the atmosphere, and plays there a very important part, attracted to the subject not only great popular attention, but enlisted as observers a multitude of students of medicine the world over, who hailed the newly-discovered body as an invaluable therapeutic agent, and rushed forward to establish by sufficiently numerous observations the relations between its presence or absence in the atmosphere, and the kind and prevalence of disease. Thirty years have passed away, and neither anticipation has been realised. Indeed, at the present hour, the possible value of ozone as a therapeutic agent is obscured by its having fallen into the hands of empirics; and the multiplication of inexact observations, and the crude and hasty generalisations therefrom, have covered with a sort of scientific opprobrium the whole subject of Atmospheric Ozone.

What causes have led to these lamentable results in the past; what prospects are there that both subjects can be reinstated in good scientific standing in the future?

And first with regard to ozone as a therapeutic agent. Without considering at present the unsettled questions of a medical character, as to the proper mode or amount or propriety of application, we apprehend that there have been hitherto three grave instrumental difficulties: — 1st. To obtain ozonised air or oxygen of known strength and of adequate purity. 2nd. It is doubtful whether in one form in which the attempt has been made to employ ozone in medicine, that of "ozonised water," any ozone whatever has been present. Such was the case with the "ozone-water" of Krebs, Kroll, and Co., in which Rammelsberg

found *chlorine*. Since ozone is so slightly soluble in water at common temperatures that it is extremely difficult to demonstrate the fact of solution, the proposition to employ "ozonised water" as a remedial agent opens a wide door to quackery. 3rd. It is certain that from the mixture of potassium permanganate and sulphuric acid, which has been and is recommended as a convenient source of ozone for medical use, no ozone, but merely chlorine and oxides of chlorine (due to impurities in the permanganate) are derived.

These errors have been exposed and the difficulties overcome. There is no obstacle to having in the office of the physician, the sick-room of the patient, or the wards of the hospital, ozonisers suitable to each place, and adequate to supply ozonised air or oxygen of known strength and purity. This being the case it remains for the therapeutist to do his part of the work, and to discover when and how ozone is to be employed in legitimate practice.

Second, to detect the amount of ozone present at any time or place in the atmosphere, and the *rôle* this atmospheric ozone plays as a disease excitant or prophylactic. The objections which vitiate the observations hitherto made are two in number:—1st. The ozonoscopes hitherto employed, Houzeau's and the thallium-test included, are all affected by some one of the gaseous bodies possibly present in the atmosphere, as well as by ozone. 2nd. The method of conducting the observations is in its nature inexact, and variations in wind, temperature, humidity, &c., are allowed to increase the resultant errors.

Advance in this direction is to be looked for only when the methods at present in use are abandoned in favour of others more in harmony with those pursued in other branches of gas analysis, and when reagents are employed which will assign true values to the amounts of ozone determined.

II. *The Nature of the Constituent Matter of Ozone.*

In his speculations upon the nature of ozone, Schönbein was far less fortunate than in his multiplied inquiries into its sources, properties, and applications. The difficulty at that time of procuring air or oxygen containing more than a minute percentage of ozone, and of manipulating it when obtained, was very great, so that precise quantitative investigations were attended with formidable obstacles, and probably for that reason were rarely instituted by Schönbein.

He brought forth a variety of hypotheses, thus introducing great uncertainty into a confessedly difficult subject, and necessitating the labours of chemists during nearly a quarter of a century for their complete overthrow.

His earliest hypothesis was that ozone is a compound consisting of hydrogen and oxygen. This, in 1844, he abandoned in favour of the theory that ozone itself is elementary, and along with hydrogen enters into the composition of nitrogen, which is a compound substance. The following year he reverted to his original hypothesis, and, while maintaining strenuously that ozone is not peroxide of hydrogen, he nevertheless upheld the view that it is composed in certain unknown proportions of hydrogen and oxygen.

The second hypothesis was overthrown by the experiments of Marignac and De la Rive, who showed that ozone could not be derived from the decomposition of nitrogen, inasmuch as they obtained it by passing electric sparks through perfectly pure and dry oxygen. They proved the resultant body to be ozone, by causing it to react on moist silver and potassium iodide with the formation of argentic peroxide and iodate of potassium. They explained these reactions by supposing that under the influence of the electric discharge the oxygen had acquired an electrified condition, with exalted chemical properties: in other words, that ozone is oxygen, and oxygen only, but oxygen in an electrified state. Plausible as was this explanation, there was nothing in the experiments—water having been present in the reaction upon silver and potassium iodide—to confute the different interpretation brought forward by Schönbein, that ozone was oxygen to which in some way was added the elements of water. Nor was this point settled by a more elaborate experiment of the same nature, instituted by Fremy and Becquerel in 1853, who demonstrated that when a certain volume of oxygen is confined over an aqueous solution of potassium iodide, moist silver, or mercury, *all* of the oxygen undergoes absorption by the reagent under the influence of a sufficiently prolonged series of electric sparks.

The first to abandon the theory that hydrogen is a constituent of ozone was Schönbein himself (1849). He employed air, ozonised as strongly as possible by moist phosphorus, and afterwards dried by passage through a sulphuric acid drying tube. That water was employed in the generation of the ozone was not from Schönbein's point of view an essential element in the problem; it was whether

this ozone after drying still contained the elements of water or hydrogen.

Three hundred litres of the desiccated air were passed through a narrow glass tube heated to redness, in order to decompose the ozone, and then through a second sulphuric acid drying tube. Since the latter in repeated experiments showed no increase of weight, Schönbein regarded the absence of hydrogen in ozone as conclusively proven. At the same time he did not accept the views of Marignac and De la Rive, declaring that to him the existence of an allotropic modification of a gaseous body was inconceivable. For a long time, however, the theory that ozone was a compound of hydrogen and oxygen prevailed. It derived great weight from the experiments which had been made by Williamson in 1845. He prepared ozone by electrolysis, and to avoid obtaining any hydrogen along with the electrolytic oxygen, used oxide of copper dissolved in sulphuric acid as the electrolyte. The gas was dried over calcium chloride, and then passed over ignited copper turnings into a second drying tube: this uniformly showed an increase of weight. The copper previous to ignition had been reduced by carbonic oxide, and not by hydrogen, in order to prevent the possibility of any occluded hydrogen being given up, on ignition, to the stream of ozonised oxygen.

These views were apparently confirmed by Baumert's experiments (1853). He passed the electrolytic oxygen evolved in such a manner as to exclude the presence of hydrogen, through a very long sulphuric acid drying tube, and thence into an absorption apparatus containing potassium iodide and provided with a sulphuric acid bulb-apparatus, to condense evaporated water. In case the matter of ozone and oxygen were identical, the weight of oxygen equivalent to the weight of iodine set free by the ozone should have been equivalent to the total gain in weight by the absorption apparatus. But, according to the experiments, this weight was less, and the numbers found apparently assigned to electrolytic ozone the formula H_2O_3. And since Baumert found that ozone prepared by the electric discharge could not be made to yield up the elements of water on strong heating, while that prepared by electrolysis could, he regarded the two as different bodies—the former as allotropic oxygen, the latter teroxide of hydrogen.

Thus the old hypothesis, against which Schönbein had so long striven, that there were two (and possibly more) bodies of the nature of ozone, was rehabilitated. It was finally overthrown by Andrews (1856), who showed that the

preceding experiments on electrolytic ozone had been vitiated by the presence of a small but appreciable quantity of carbonic acid, which, unless very great precautions be taken, is always present in the evolved gas. In very numerous experiments he showed that the weight of active oxygen equivalent to the weight of the iodine set free in the absorption apparatus, was equal to the entire gain in weight of the apparatus, and therefore no hydrogen as well could have been present; also that the properties of electrolytic ozone, and that obtained by the action of the electrical spark on pure and dry oxygen, were identical. More especially, it was shown that both were converted into ordinary oxygen at a temperature of about 237° C.; and from the whole investigation the author drew the conclusion, which was confirmed by the still more elaborate experiments of Soret in 1863, and is now universally adopted, "that ozone, from whatever source derived, is one and the same substance, and is not a compound body, but oxygen in an altered or allotropic condition."

III. *The Exact Nature of the Relations existing between Ozone and Ordinary Oxygen.*

We have seen that Marignac and De la Rive, as the result of their experiments performed in 1845, had enunciated the view that ozone was oxygen in a peculiar electric state. They proposed to abandon the name "ozone," which assumed an independent chemical existence for this body, and to call it merely "electricised oxygen." This view of the constitution of ozone was one not readily susceptible of investigation by usual chemical methods. But the case was different with the hypothesis which was shortly afterwards advanced by Dr. T. Sterry Hunt, in 1848. Since his intuition of a truth, not fully demonstrated until twenty years later, is of a very striking character, it will be interesting to quote it as originally announced. In a paper on the anomalies presented in the atomic volume of sulphur and nitrogen, Dr. Hunt says—"In considering such combinations as SO_2 and SeO_2, which contain three equivalents of the elements of the oxygen group, it was necessary to admit a normal species which should be a polymer of oxygen, and be represented by $O_3 = (OOO)$. The replacement of one equivalent of oxygen by one of sulphur would yield sulphurous acid gas (OOS), and a complete metalepsis would give rise to (SSS). The first compound is probably the *ozone* of Schönbein, which the late researches of Marignac and De la Rive

have shown to be in reality only oxygen in a peculiarly modified form, &c."

The hypothesis herein stated, that ozone is triatomic oxygen, necessarily involved the assumption of such a corresponding difference in density, and other physical properties,—differences admitting of exact quantitative proof or disproof. Such were the experimental difficulties in the way, however, that it was not until 1860 that an investigation was made into the volumetric relations of ozone to oxygen. The experiments of Professors Andrews and Tait then resulted in establishing that when perfectly pure and dry oxygen is converted into ozone, under the influence of the silent electric discharge, it becomes more dense, the amount of contraction being proportional to the quantity of ozone produced; also that when ozone thus condensed is exposed for a short time to a temperature of 270° to 300° it expands to its original volume. That the increase in density was exactly proportional to the amount of ozone formed was proven by an analysis of the contracted gas by means of potassium iodide. The amount of iodine in every case set free was precisely equivalent to the weight of a volume of oxygen equivalent to the volume of the contraction which the oxygen had experienced in the process of ozonation. The same laws were demonstrated to hold good with regard to electrolytic ozone, not only by these authors (1860), but also by Von Babo and Claus and by Soret (1863).

Andrews and Tait found great difficulty in reconciling the theory of the allotropism of ozone with their experiments, inasmuch as the oxidation of a body like mercury, potassium iodide, &c., was effected without any diminution in the volume of the contracted gas. In other words, the density of the allotropic oxygen concerned in this oxidation was apparently infinite. They sought therefore to explain the origin of ozone by the assumption of a decomposition of the oxygen.

But in 1861 Odling put forth the interpretation that ozone was a compound of oxygen with oxygen, the combination being attended by a contraction. Hence if one portion of the combined or contracted oxygen were absorbed by an oxidisable body, the other portion would be set free, and by its liberation might expand to the initial volume. He likewise suggested that this contraction might consist in the condensation of three volumes of oxygen into two volumes, not because this ratio was the only one which would explain the volume and density relations, so far as then known, but because, on the hypothesis of the dual nature of oxygen,

this was their simplest possible explanation. Four years later Soret discovered that a very remarkable reaction occurs when electrolytic ozone is allowed to act upon oil of turpentine. Its volume is diminished by a volume equivalent to twice that of the oxygen, corresponding to the iodine set free on passing the ozonised oxygen into a solution of iodide of potassium. The latter, it will be remembered, is the same as the diminution in volume which the oxygen undergoes in ozonation, and may be called the contraction-volume. Hence the two volumes of ozonised oxygen absorbed in Soret's experiments contained not only their own volume of oxygen, but that contained in the contraction-volume, or in all three volumes of ordinary oxygen. The density of ozone, therefore, was to the density of oxygen as three to two, or 1·6584, the density of ordinary oxygen being 1·1056.

Soret inferred rather than demonstrated these relations, inasmuch as in his first set of five experiments the ratio of the total volume of ozonised oxygen absorbed by the turpentine to the contraction-volume was 2·4, and in his second set of seven experiments 1·81, both of these results being far from 2, the theoretical number.

However, in 1872, Sir Benjamin Brodie, by the introduction of methods of exact volumetric character, supplied a rigorous experimental demonstration. He obtained in a set of eight concordant experiments made with oil of turpentine, for the ratio between the whole diminution in the volume of the original oxygen, to the diminution in the volume of the ozonised oxygen, as a mean result, 3·02 to 2·02. Operating in the same manner with a neutral or slightly alkaline solution of sodium hyposulphite, he obtained, as a mean result of twenty-seven concordant experiments, the same ratio of 3·02 to 2·02. In these experiments the actual weight of the oxygen absorbed could not be determined otherwise than by calculation from the alteration in volume. But by the oxidation of stannous chloride, under proper conditions, he effected a direct determination, and found that the weight of the oxygen absorbed from the ozonised oxygen by the stannous chloride was almost exactly three times the weight absorbed from the same gas by potassium iodide. At the same time the volume in the first case was almost exactly twice the contraction volume, as determined by the latter reagent.

II. SALT INDUSTRY OF ENGLAND.

I. Brine Pits.

TWO methods generally are adopted for obtaining salt supplies in this country, viz., from brine springs and from rock salt. The former is by far the more ancient of the two, as will be presently shown. At the earliest time of which we possess records on the subject wherever there was a brine spring or a manufacture of salt, the place appears to have been called *Wich*. Thus the name Droitwich was probably originally *Wich*, and it is supposed that the prefix *Droit* was given to designate a certain legal or allowed brine pit. We have also Nantwich, Middlewich, Shirleywich, and so on.

Some of the earliest records of the brine springs relate to those of Droitwich. From these it appears that in the year 816 Kenulph, King of the Mercians, gave Hamilton and ten houses in Wich, with salt furnaces, to the Church of Worcester; and about the year 906, Edwy, King of England, endowed the same church with Fepstone and five salt furnaces, or scales. In Domesday Book, when, between the years 1084 and 1086, William the Conqueror caused an inquiry to be made into the names of the several places, by whom they had been held in the time of Edward the Confessor, the last hereditary Saxon king, and who held them when the inquiry was made, the Wiches and salt houses then in operation are respectively recorded; and it is clear that in these times, as regards Cheshire and the detached parts of Flintshire called Maylor, the rights of property were fully exercised over the brine springs and salt works, and and that there then existed certain well defined customs with regard to them. In King Edward's time there was a Wich in Warmundestron hundred in which there was a well for making salt, and between the king and Earl Edwin there were eight salt houses, so divided that of all their issues and rents the king had two parts and the Earl the third. From our Lord's Ascension to Martinmas anyone having a salt-house might carry home salt for his own house; but if he sold any of it either there or elsewhere in the county of Cheshire, he paid toll to the king and the earl. Whoever after Martinmas carried away salt from any salt-house except one devoted to the earl's private use paid toll, whether the salt was his own or purchased. In the year 1245 it is

recorded that Henry the Third caused the brine springs to be destroyed to prevent the Welsh, with whom he was at war, from getting supplies of salt. The brine springs of Cheshire were described in 1808 as being found in the valleys of the Weaver and Wheelock, and nowhere else except at Dirtwich, on the border of the detached part of Flintshire, and a weak spring at Dunham, near the river Bollin. In the Dane valley springs are also supposed to have once existed in the neighbourhood of Congleton, as some of the inclosures were named Brinefield, Brinehill, &c. In addition to these places, Mr. W. H. Ormerod, in 1848, on the salt field of Cheshire, adds discoveries of brine at Acton, Broadlane, and Hatherton, near Nantwich, the site of the viaduct of the Manchester and Crewe Railway over the Wheelock, Red Lane, Elton, the Flint Mill near Middlewich, the west side of Hartford Bridge, Eaton, opposite Vale Royal, and the finding of brackish water at Minshall Vernon. At present the brine springs worked in Cheshire are in the same valleys as in the olden times, but the principal works are confined to the lines of the river, canal, and railway communication. Brine still flows to the surface at Brine Pits Farm and at Shewbridge, between Audlem and Nantwich; but in 1808 there were no manufactures above Nantwich; and at Nantwich itself, one of the most ancient of the Wiches, salt ceased to be manufactured about the year 1847. At Dirtwich, the higher Wich on the Cheshire side ceased to be worked about the year 1830, and the lower Wich, in Maylor hundred, Flintshire, ceased about the year 1856. In Staffordshire, at Shirleywich and Weston-upon-Trent, brine has been used from early times. There appear to have been numerous ancient pits along the banks of the Trent, but the top water having got in, they became waste. In modern times, the late Mr. Stevenson, C.E., was called in to advise, and he put down a shaft and tubed it with iron castings; but this also proved a failure, and at present there is only one shaft at Weston and one at Shirleywich. In Shropshire, at Adderley Wood, between Adderley and Audlem, a trial shaft was sunk in the blue lias and perforated into red ground. The water found in this shaft was salt, as also in another shaft close by which had been sunk many years before. Brine springs have also been found in other parts of England: as in Somersetshire, Westmoreland, Durham, Lancashire, and Yorkshire; but they were either weak, like those occasionally met with in coal mines, or were situated where fuel was scarce, so that they have not been much noticed.

Until the discovery of rock salt, it was only supposed that brine springs were formed by the solution of rock salt. It is now known that water permeates from the surface, and that, until recently, all the brine springs came from the top of the rock salt, or as it is locally termed, the "rock-head." In addition to this source of supply, brine is now obtained in large quantities in the neighbourhood of Northwich, from the old rock-salt mines, into which water from the surface, together with brine from the rock-head, finds an entrance. The weak solution coming in contact with the old pillars and the remainder of the rock salt that was left unworked, is formed into pure brine; and the chambers being pierced where the brine is fully saturated, the pumping goes on without the separating barrier being dissolved. There are, therefore, two sets of modern brine pits in existence, those of the rock-head and those of the inundated old workings.

The precautions necessary for securing the brine shafts are, in many respects, identical with what is required in the rock salt pits, and having been earlier in point of time, the necessities seem to have been met as they arose. It seems that in sinking to many of the springs the supply of brine, when cut into, was so copious that the sinkers had to escape for their lives, sometimes rising up the shaft amongst the brine without any opportunity being afforded of seeing what was underneath, which accounts for the lateness of the discovery of the rock salt. In these sinkings, when it is still unknown at what depth the brine is likely to be met with, there seems to be no entire remedy against these sudden entries of brine. But in the proved districts it is now observed that before reaching the top of the rock salt, where the rock-head brine flows, there is often a bed of hard marlstone, called "the flag," and that for a few feet above it the marl is of a granular structure, called "horsebeans." Therefore, when these indications are observed, and the brine is expected to be found at a high pressure, the practice is to case the shaft sides carefully down to the flag, to keep them secure, and prevent surface water from entering. The flag is then either blown through with powder, or bored through with boring rods. One of the best methods of tapping the brine, when under pressure, is to sink the shaft nearly as deep as to where the brine is expected, and then to case it with iron cylinders, having an iron bottom to the lowermost cylinder, with two holes in it to which pipes may be attached. From each of these two pipe holes a column of pipes, of about four inches in diameter, is erected inside the cylinders, either to the top, or as high as the brine is

expected to rise, the bottom pipe having holes in it to let out the brine when it is tapped. Being thus equipped, a set of boring rods is let down each pipe, and the remaining strata at the bottom of the cylinders are bored through into the brine. On being thus tapped, the brine rises up the bore hole, and, entering the pipe, passes through the holes near the bottom into the cylindered shaft, where it rises to its level. Provision is also sometimes made by having a plug, or a tap, at the bottom, below the holes in each pipe, so that if needed the entry of the brine into the shaft may be stopped, and the shaft emptied. In the brine shafts, where brine is pumped out of the old rock salt mines and is met with at a much higher pressure than in the rock-head brine shafts, the tapping has been attended with extraordinary difficulties. The brine in these old workings rises to as high a level as the brine that is found at the rock head, and as it has to be tapped through a pillar near the bottom of the old workings, the pressure is proportionately higher.

It seems to have been long noticed that in Cheshire the Northwich brine contained a trace of iron, and that the earthy salts were the same which were held in solution by sea water, being principally chloride of magnesium and sulphate of lime; the proportion of the earthy salts to pure chloride of sodium in sea water being greater than that which prevailed in the brine. Analyses made in 1808 of various brine springs showed that the percentage of chloride of sodium and of earthy salts in one pint of brine varied from 26·566 to 21·250 of the former, and from 2·500 to 0·625 of the latter.

The brine, on being pumped from the pits, is run into large cisterns, or into reservoirs, made sufficiently high for the brine to flow by gravitation through pipes as it is required in the evaporating pans. It is there evaporated upon one general principle. The heat for the evaporation is usually supplied from coal fires beneath, but sometimes the spare heat from a steam boiler is used after it has passed the boiler, or the discharged steam from an engine is sometimes utilised in the same way; and, occasionally, there are pipes with steam in them amongst the brine in the pans. In this way, according to the different degrees of heat applied, the product is large or small grained salt. For what is called lumped, or fine grained salt, the brine in the pan is brought to a temperature of 226° Fahrenheit, which is the boiling point for brine. Crystals soon form on the surface, and after skimming about a little they subside to the bottom. Each crystal appears granular or a little flakey, and is in

the form of a small quadrangular though irregular pyramid. For common salt the temperature is 160° to 170°. The salt thus formed is close in texture and clustered together in larger or smaller pyramids according to the heat applied. For large-grained flakey salt the temperature is 130° to 140°. For large grained fishing salt the temperature is 100° to 110°, the slowness of the evaporation allowing the salt to form in large cubical crystals, although it appears they are not perfect cubes. To produce these kinds, foreign matter supposed to be of a harmless kind, such as the white of eggs, calves and cows feet, ale, flour, resin, butter, alum, &c., have long been added to the brine for clarifying and to promote crystallisation.

The earthy matter contained in the brine is got rid of in the manufacture by its adhering to the pans in the form of scale, called pan-scale, or pan-scratch. There is also the chloride of magnesium, called bittern, which remains in solution after the chloride of sodium, or common salt, is formed. This is often purposely allowed to flow away by having the floor, called the hurdles, on to which the salt is lifted from the pans, lower than the top of the pan. The pans are of various sizes, the only limitation being that they must not be too wide for a man to draw out the salt with a ladle. They are commonly made of wrought iron, three eighths of an inch in thickness, and about 50 or 60 feet in length by 24 or 25 feet in breadth, and 2 feet in depth; but some of the new pans are 140 feet by 30 feet by 2 feet.

In the early manufacture of salt, it appears that evaporation was by the heat of the sun, and the operation of the air. The brine or sea water was run into shallow pits or reservoirs, where it evaporated to a certain degree, and was afterwards completed by pouring it upon twigs, and sometimes, it is said, by pouring it upon burning wood, and collecting the salt deposited upon the ashes. Until long after historic times wood was the only fuel used, and the large consumption for this purpose seems to have been early complained of. It was not until the year 1656 that the substitution of coal at Nantwich is mentioned as a novelty. The brine springs, which flowed naturally into the valleys, would probably be used first, and when they began to fail, or become insufficient for the increasing wants, it appears they were followed down with buckets and with pumps worked by hand, water-wheel, and windmill, until now, when the only method used for pumping is the steam engine.

III. THE MARTYRDOM OF SCIENCE.*

By J. W. SLATER.

THE history of human progress presents no feature more interesting yet more commonly overlooked and misrepresented than the treatment of discoverers and inventors. That these men have, as a rule, fared ill at the hands of their species is carelessly or grudgingly admitted. But the questions by whom have they been persecuted, and what may have been the motive of their enemies, are avoided even in works where we might expect them to be carefully discussed and fully answered. Such omission may be especially charged against Sir D. Brewster. His treatise is merely a biography of certain astronomers who have been, for anything the reader learns to the contrary, incidentally and casually afflicted by their contemporaries, and it omits the most striking instances of persecution. Nay, the very term "martyrs of science" is applied quite vaguely, and is made, *e.g.*, in the work of M. Tissandier, to include three distinct classes of men. We have on the one hand personages whose love for research has cost them health and even life itself. We find physicists like Richmann, chemists like Gehlen, Mansfield, Chapman, who have been struck dead whilst engaged in some hazardous experiment. We read of naturalists like Marcgrave and the elder Wallace; geographers, navigators, and travellers, such as Cook, La Perouse, Franklin, Livingstone; meteorologists like Crocé-Spinelli, who in their ardour for discovery have succumbed to ungenial climates, to the attacks of savages, to hunger, tempest, or to an irrespirable atmosphere. All honour to these men, and to the noble army of which they may be taken as representatives. They have fallen in the cause of science, but they have undergone no persecution, and may hence be regarded as victims rather than martyrs.

We turn to another class: illustrious inventors and discoverers not a few have been clearly and decidedly persecuted; hunted down by mob-violence, imprisoned, or even judicially murdered; but these inflictions are to be traced

* Les Martyrs de la Science. By GASTON TISSANDIER. Paris: Maurice Dreyfous.
Heroes of Invention and Discovery. Edinburgh: W. P. Nimmo and Co.
The Martyrs of Science. By SIR D. BREWSTER.
History of the Inductive Sciences. By PROF. W. WHEWELL.
Hypatia. By the REV. C. KINGSLEY.

not to their scientific discoveries, speculations, and writings, but to their religious or political opinions. When the house of Priestley was sacked and burnt by the rabble of Birmingham, and when his very life was endangered it was not the chemist and physicist but the so-called "Jacobin" and Socinian whom Midland roughdom sought to crush. It is not, we believe, generally known that the attack on Priestley's house was headed by the town-crier, a man of the name of Sugar, who rang his bell and exclaimed:—

> "Pile up the wood higher,
> I am Sugar, the crier;
> By my desire
> This place was set on fire!"

This man and his doggerel are only worth our notice as proof of the official countenance lent to the outrage. It is utterly incredible that a town crier would thus avowedly act as the ringleader of a mob unless sure of the connivance of his superiors.

If Campanella was put seven times to the torture, on one occasion for forty hours in succession; if he passed twenty-seven of the best years of his life in loathsome dungeons; if, after his release, he narrowly escaped the rage of a brutal populace, it was not as the champion of the Copernican system of astronomy, the refuter of mediæval Aristotelianism, but as a patriot who longed to deliver southern Italy from the tyranny of Spain, that he suffered. Still we may concede that like all the reformers of science he must have aroused the hatred and jealousy of many of the learned, who would doubtless use against him whatever influence they possessed.

Servetus was certainly a learned physician, and is by some ranked as one of the forerunners of Harvey. But his judicial murder by Calvin was due solely to his theological opinions. The merits of Bernard Palissy, not merely as the creator of modern fictile art, but as an able physicist, chemist, and geologist, cannot be contested. He shocked the philosophasters and sophists of his day by maintaining that fossil shells were not, as was then supposed, mere freaks of nature, but the remains of extinct animals. He dared to deny that stones were capable of growth. He pointed out the possibility of artesian wells. With an almost prophetic insight he foretold the evil consequences of the destruction of forests, and in our day not merely meteorologists and farmers, but governments find that he was in the right. But in spite of all his innovations in science and in industrial art—or rather in consequence of

those very innovations—he was honoured and protected by Catherine of Medicis and Henry III. That he was at last arrested, condemned to death, and allowed to die in the Bastille, was the consequence of his firm adherence to the doctrines of the Huguenots. Had it not been for his scientific greatness he would have perished earlier.

If Lavoisier perished on the scaffold amidst the storms of the first Revolution, he merely shared the fate of his colleagues the *fermiers généraux*, none of whom were men of science. It is true that "the brutish idiot into whose hands the destinies of France had then fallen"—as Prof. Whewell justly remarks—declared that "the republic had no need of chemists." But these foolish words give us no right to assert, as a modern writer has done, that Lavoisier suffered death for his chemical ideas.

If Bailly likewise perished upon the scaffold, and if Condorcet poisoned himself to escape a similar fate, they died not as philosophers and mathematicians, but as victims of indiscriminate popular frenzy.

There are many other men whose names we are thus compelled to erase from the list of the martyrs of science—men whose inventions and discoveries have been of the highest order, but whose sufferings and death cannot be justly looked on as a consequence of their achievements.

But there still remains a third and a too numerous class: thinkers and discoverers who have been persecuted in many cases to the death, not incidentally but because of the very services they have rendered to science. Their persecutions have differed very much in nature and degree according to the age and the country in which they lived. In the dark ages it was practicable to arrest a troublesome thinker, and to put an end to his researches, or at least to their promulgation, by the straightforward means of imprisonment, torture, banishment, and even death at the stake. Hypatia of Alexandria was seized by a mob of infuriated monks, who literally tore the flesh from her bones with fragments of pots, dragged her mangled remains outside the city, and there burnt them. The Bishop Cyril, who had instigated the outrage, endeavoured to screen the malefactors from justice. Virgilius, Bishop of Salzburg, was burnt by Boniface, the Papal legate, for asserting the existence of antipodes. Cornelius Agrippa, after much persecution, died at last of actual famine. Roger Bacon, perhaps the mightiest philosopher of the middle ages, of whom it has even been said that could he revisit the earth he would shake his head at the slowness of our progress since his death, suffered bitterly. He was

first prohibited from lecturing at the University of Oxford and from communicating his researches to any one. The accession of Clemens IV. to the Papal chair gave the illustrious sage a short respite, of which he availed himself to draw up three works, and to publish one of them, the *opus majus*. Scarcely was this effected when the enlightened pontiff died, and his successor was indifferent, if not formally hostile. Roger Bacon was summoned to appear at Paris before the legate Jerome of Ascoli, was convicted of heresy and witchcraft, and sentenced to imprisonment for life. His works were also condemned as impious, and all persons were forbidden to read them under pain of excommunication. It is certain that he remained ten years in a loathsome dungeon, and that his treatment, even in that rude age, was considered exceptionally harsh. Some say that he died in prison; others, that he was at length set free at the intercession of certain powerful nobles, and ended his days in England. He is said to lie buried at Oxford. We can wish that ancient university no greater boon than that his spirit may ever rest upon its professors.

Three centuries later Rome witnessed one of the foulest murders ever committed. Giordano Bruno, for upholding the teachings of modern astronomy, and especially for maintaining the immensity of the universe and the plurality of worlds, was burnt to death in the Campo di Fiore on February 16, 1600. The words of the sentence passed upon him are significant:—"Ut quam clementissime et citra sanguinis effusionem puniretur." Not less memorable was the reply of the hero-philosopher: "You feel more fear in pronouncing this sentence than I do in receiving it!"

One of the greatest merits of Bruno is his enunciation of the doctrine that on all scientific questions the Scriptures neither possess nor claim any authority, but embody merely the opinions current at the times when they were written. This proposition, from which follows as a corollary that the Church can have no claim to pronounce on the truth or falsehood of scientific theories, was afterwards enforced at length by Galileo in his celebrated letter to the Dowager Grand Duchess Cristina of Tuscany. We cannot help regretting that he, when brought before the Inquisitors in the Convent of Minerva, did not act up to his profession by denying *in toto* the authority of the court. Had he done so his life would doubtless have been in great peril, but the enemies of science would have been deprived of much scope for sophistry. "E pur si muove" was well, but "non coram judice" would have been infinitely better. It is worthy of

note that, unless we are misinformed, St. Augustine had warned the clergy against the attempt to exercise a jurisdiction over science.

As we approach modern times a change becomes manifest. Ecclesiastical bodies in the more civilised parts of Europe were deprived of civil power, and could no longer imprison, torture, or burn inventors and discoverers. But the old spirit faded away very slowly, and even in our days it still occasionally comes to light. Men of science, scientific works, and learned societies were, and still are, traduced, denounced, and held up to public hatred. Scarcely a capital step has been taken in any branch of research but it has been branded as atheistic. Dean Wren, the father of the celebrated architect, upheld the geocentric theory of the universe and the immovability of the earth in a strain worthy of Caccini or Scioppius. It was objected against the Royal Society that its members neglected the wiser and more discerning ancients and sought the guidance of their own unassisted judgments, and that by admitting among them men of all countries and religions they endangered the stability of the English Church." It was urged that experimental philosophy was likely to lead to the overthrow of Christianity and even to atheism. Among these writers a prominent place belongs to Henry Stubbs, of Warwick, and the Rev. Richard Cross, of Somerset, the latter of whom charged the Fellows of the Royal Society with "undermining the universities, destroying Protestantism, and introducing Popery!"

It would have been fortunate for Bruno, Galileo, and not a few of their colleagues, if the Inquisition and the Order of Jesus had taken the same view of the tendency of their researches. The discoveries of Sir Isaac Newton excited an outburst of hostility very similar to that which has in our own times greeted the theory of organic evolution. Then geology became the great bugbear, then followed the nebular hypothesis, till, as we have just hinted, anti-scientific jealousy concentrated itself upon the views of Darwin, Wallace, and their followers. If we read the controversial literature which has issued from the English press within the last half century, and note the motives therein imputed to men of science, we can scarcely doubt what would have been the fate of Buckland, Lyell, Sedgwick, Oken, Carus, Richard Owen, Darwin, had their enemies possessed as much power as malice. It must also be remembered that the practical applications of science, and all attempts at its extension among the public, have been

met with an hostility no less pronounced. Franklin's lightning conductor, and Jenner's discovery of vaccination, have been condemned from the pulpit as impious and blasphemous attempts to set aside the decrees of heaven. A similar condemnation has since been pronounced against the use of anæsthetics, especially in midwifery.

The late Society for the Diffusion of Useful Knowledge, the London University, and the British Association, have each in turn passed through a tempest of abuse. The last mentioned body, indeed, is still regularly "preached at" in every town which it visits.

In France the Chancellor, D'Aguesseau, refused a licence to print Voltaire's "Letters on England," because the author therein expounded the discoveries of Newton, and disproved the vortex theory of Descartes. For adopting Locke's denial of innate ideas, a "lettre de cachet" was issued against Voltaire, and he was compelled to seek safety in flight. More recently the freedom of science seems to be recognised in France, Germany, and even in Italy. We must not, however, forget that the Roman Church has never formally retracted her claim to adjudicate upon scientific truth. An "Index," of proscribed books is still issued, and within the present century Pope Gregory, in an encyclical letter, characterised the freedom of the press as "deterrima illa ac nunquam satis execranda et detestabilis libertas artis literariæ."

In Britain the anti-scientific spirit still lingers more decidedly than elsewhere. Its chief lurking-places are sometimes said to be among the clergy and country gentlemen. We are not sure that this view is correct. Passing through a street in one of our northern manufacturing towns, the present writer once heard a demagogue addressing a crowd on something which he contended must be put down. That something was science! We are bound to say that his listeners gave every mark of sympathy and approval. The manner in which inventors have often been treated in different parts of England seems to show that such feelings are widely spread. The country which first wins over her working-classes to favour invention, and to become themselves inventors, will command the industrial supremacy of the world. America is fast attaining this object by her patent system, which enables even a poor man to secure his property in an invention. Our statesmen, Whig and Tory alike, can scarcely be restrained from laying additional difficulties in the way of patent right.

If we now, summing up, seek to know who have been the

chief persecutors of science, we shall find the conventional answers too narrow. Many persons have laid the chief blame upon Roman Catholicism. It is very questionable, however, whether other churches, if they had been as widely spread, and had possessed as great civil power and authority, might not have equalled or even exceeded Rome. The religious bodies of Britain, established or dissenting, have certainly been unsurpassed in the virulence of their attacks upon geology and upon the New Natural History. We strongly suspect that the Church of Rome will be the first religious body to admit that the doctrines of evolution and of the high antiquity of the human race are not necessarily opposed to the teachings of the Scriptures. So-called infidels of various grades of opinion have contended that Christianity in any and every form is the persecutor of science. We would submit, on the contrary, that discovery was persecuted in heathen and democratic Athens, where all the influence of Pericles barely sufficed to save his friend the philosopher Anaxagoras from a worse fate than banishment. Nay, we may even venture to predict that modern "freethought" will before long appear as the adversary of science, and if sufficiently powerful, as her persecutor.

The jealousy of the industrial classes we have already glanced at.

Lest we should feel tempted to ridicule the suicidal folly of the working-classes in thus seeking to repress improvement, let us remember that science is sometimes her own persecutor. Men who have gained a high official position in universities and academies are often actuated by a jealousy very similar to that which we have traced among ecclesiastics. They establish a certain scientific orthodoxy, based often to a great extent on mere conjecture and assertion, and seek to frown down and to silence the unknown outsider who calls in question one of their dogmas, or who discovers a truth which they have overlooked. That any region of research should be officially tabooed is a humiliating circumstance. The dread of truth, the jealousy of discovery, is not confined to the Holy Inquisition, and no disestablishment of Churches, no secularisation of schools and colleges, not even the suppression of every religion—were such a step possible—would put an end to its action.

IV. THE "EGYPTIAN" ARGUMENT ON THE ORIGIN OF SPECIES.

By J. W. SLATER.

AMONG the scientific results of the French Expedition to Egypt, under the first Napoleon, was a series of observations of the birds and mammals sculptured on the most ancient monuments or actually preserved in the state of mummies. On a comparison of these images and remains with their living representatives as existing in our own days, a very close resemblance, or perhaps even an absolute identity, was perceived. The sacred ibis of this nineteenth century might have served for the original of the representations of his ancestors chiselled out at least three thousand years ago. Hence the inference was drawn by the Cuvierian or official school of French naturalists, that, if the characteristic features of animals undergo no change in so long a time, species must be regarded as incapable of transmutation, and their respective differences must be absolute and primordial.

As this contention has lately been resuscitated by Dr. F. Bateman, of Norwich, it may need a brief reconsideration.

It cannot escape our notice that this argument rests upon certain assumptions which, to say the least, have never been proven. The Cuvierians take for granted that because a certain phenomenon cannot be shown to have happened within a certain interval of time, and in one particular locality, it can never have happened at all. They tacitly hold that the modification of species, if it occur at all, must proceed constantly at a uniform rate, at all times and in all places. They assume that the space of three thousand years forms a sufficient portion of the life of the organic world to decide the question. They forget that in Egypt the climate and outward circumstances generally have undergone no perceptible change within historical ages.

All these considerations have been urged by various writers with a view of showing that the evidence of the old Egyptian monuments cannot be taken as decisive.

But there is a further objection which, so far as I am aware, has not yet been raised, and which to me appears utterly fatal. If we take a further glance at the oldest

Egyptian monuments we find there pourtrayed men of different races, or, if the term is preferred, varieties, possessing the very same distinctive characteristics which we observe in their descendants in our own times. The negro of to-day is the exact counterpart of the negro figured in the most ancient of these sculptured records.

If, then, permanence of type for the term of let us say three thousand years proves that certain animal species are immutable and primordially distinct, surely the same conclusion must be extended to the varieties of the human race, and, *e.g.*, the Negro, the Arab, and the Jew must be traced each to a distinct and independent origin. But if M. Flourens and Dr. Bateman consider that these well-marked —and, as we have just seen, permanent—forms have been produced from one common stock, it is hard to see how they can consistently regard a similar permanence in other animal forms a proof of radical diversity and of unalterable fixity.

V. PROGRESS IN MICROSCOPICAL SCIENCE.*

By W. T. Suffolk, F.R.M.S.

THE Fellows of the Royal Microscopical Society are certainly possessed of a "Journal of which any learned body might be proud: the first volume, issued at the end of 1878, merely professed to be a Journal of the Society," with other microscopical information. In the present volume a most important addition has been made. No less than two hundred and forty-eight English and foreign scientific periodicals are regularly examined, and their contents —so far as they come within the scope of the Royal Microscopical Society's work—indexed under the author's names. The value of this arrangement to students scarcely needs comment: the difficulty of knowing what has been written and where to find it is one of the rocks ahead to all those

* Journal of the Royal Microscopical Society, containing its Transactions and Proceedings, and a Record of Current Researches relating to Invertebrata, Cryptogamia, Microscopy, &c. Edited, under the direction of the Publication Committee, by Frank Crisp, LL.B., B.A., F.L.S., one of the Secretaries of the Society; assisted by T. J. Parker, B.Sc., A. W. Bennett, M.A., B.Sc., and F. J. Bell, B.A., Fellows of the Society. Vol. ii., 1010 pp. London: Williams and Norgate. 1879.

engaged in any kind of scientific research. The "record of current researches" has also been greatly extended, and the contents of the whole volume rendered most acceptable by copious indexing, which takes the following forms:—A carefully arranged table of contents occupying eighteen pages, lists of plates and woodcuts, and a very full alphabetical index, in which the repetition of subjects under different letters has not been spared. The energetic Secretary of the Society, finding the work of conducting such a publication beyond the power of any single individual, has called to his aid the three gentlemen mentioned in the title, whose united efforts have made the work an almost perfect record of microscopical and biological research.

The Royal Microscopical Society, desirous of making others partakers of its good things, has elected the presidents of eighty-six kindred societies, at home and abroad, *ex officio* Fellows: these, with the numerous Honorary Fellows, place the Society in communication with observers in most places throughout the world.

A careful series of experiments, to determine the thermal death-point of known Monad germs when the heat is endured in a fluid, have been carried out by the Rev. W. H. Dallinger, F.R.M.S., so well known for his researches in the life-history of Monads, in conjunction with Dr. John James Drydale, of Liverpool. The processes employed are necessarily elaborate, and need special appliances for the purpose of microscopical observation, the whole of which are described in detail in the paper communicated to the Royal Microscopical Society.* As might be expected the adult organisms were less capable of enduring a rise of temperature than the germs, a heat of from 140° to 142° F. being always fatal. With respect to the germs, development took place—after an exposure to 265°—about three and a half hours after the heating process: a temperature above 267°, however, proved fatal to the germ. The heat endured dry was much greater, a spore germinating after exposure to a temperature of 250°, a temperature of 212° in fluid being fatal.

A paper by Mr. J. W. Groves, on the "Preparation of Stained Sections of Animal Tissues," appears in the "Transactions of the Quekett Microscopical Club" (vol. v., p. 231). The author remarks that although the staining of tissues by the more simple dyes is very easy, yet of the large number prepared scarcely any are ever so stained as to show

* Journal R. M. S., vol. iii., p. 1. January, 1880.

the structures to the best advantage; indeed many of them are utterly useless. The subject is treated of under the heads of "Material—Methods of Preserving and Hardening it," "Cutting of the Sections," "Tingeing Agents," "Method of Staining." The paper is valuable on account of the large number of formulæ given, and the numerous practical hints upon points where the experimenter is likely to meet with difficulties or take the wrong way of operating. The whole concludes with the following summary:—

"1. Let the material be quite fresh.

2 *a*. Take care that the hardening or softening fluid is not too strong.

b. Use a large bulk of fluid in proportion to the material.

c. Change the fluid frequently.

d. If freezing be employed take care that the specimen is thoroughly frozen.

3 *a*. Always use a sharp razor.

b. Take it with one diagonal sweep through the material.

c. Make the sections as thin as possible, and—

d. Remove each one as soon as cut, for if the sections accumulate on the knife or razor they are sure to get torn.

4 *a*. Do not be in a hurry to stain, but—

b. Remember that a weak colouring solution permeates the section better, and produces the best results; and—

c. That the thinner the section is the better it will take the stains.

5 *a*. Always use glass slips and covers free from scratches and bubbles, and chemically clean.

b. Never use any but extra thin circular covers, so that the specimens may be used with high powers.

c. Always use cold preservatives, except in the case of glycerine jelly, and never use warmth to hasten the drying of balsam or dammars, but run a ring of cement round the cover.

6. Label the specimens correctly, and keep them on the flat and in the dark."

Herr Zeiss, of Jena, has produced a new low-power objective, which, by altering the distance of its combinations by means of a rotating collar, gives a range of magnifying

power varying from that of a glass of 4 inches focus to one of 2 inches. This means of varying the power at pleasure is valuable in dissection, especially when used with an erecting binocular microscope.

VI. OFFENSIVE AND DEFENSIVE SUBMARINE WAR.*

By Capt. S. P. Oliver, (late) R.A.

Part I.

In quaint old "Pepys's Diary," under date of March 14, 1662, we find the following entry:—"Home to dinner. In the afternoon come the German Dr. Knuffler to discourse with us about his engine to blow up ships. We doubted not the matter of fact, it being tried in Cromwell's time, but the safety of carrying them† (*sic*) in ships; but he do tell us, that when he comes to tell the King his secret, (for none but the Kings, successively, and their heirs must know it,) it will appear to be of no danger at all. We concluded nothing: but shall discourse with the Duke of York to-morrow about it." And again, in 1663, as follows:—"Novbr. 11th. At noon to the Coffee-house where with Dr. Allen some good discourse about physick and chemistry. And among other things I telling him what Dribble the German Doctor do offer of an instrument to sink ships; he tells me that which is more strange, that something made of gold, which they call in chymistry '*Aurum Fulminans*,' a grain, I think he said, of it put into a silver spoon and fired, will give a blow like a musquett, and strike a hole through the silver spoon downward, without the least force upward; and this he can make a cheaper experiment of, he says, with iron prepared."—("Memoirs of Samuel Pepys, Esq., F.R.S." Edited by Lord Braybrooke.)

The doctors "*Knuffler*" and "*Dribble*," probably partners if not identical, do not seem to have been more successful

* Torpedoes and Torpedo Warfare, by C. W. Sleeman, Esq., late Lieut. R.N., and late Commander Imperial Ottoman Navy. Portsmouth: Griffin and Co. 1880.

† "*Them*," *i.e.*, the said engines, which were evidently buoyant torpedoes.

than Lord Dundonald was, nearly two centuries later, in introducing their infernal machine to the notice of the Admiralty under the Duke of York, whose secretary Pepys was, for we hear no more mention of this important secret which could only be communicated to royalty and heirs apparent. It is fortunate indeed that Mr. Whitehead has not made this condition a portion of his compact, preferring a royalty in hard cash for his non-exclusive secret; for otherwise we should be obliged to send our young princes through a course of torpedo instruction at Fiume, preparatory to their having command of the *Vernon** and *Hecla*,† *et hoc genus omne*, at Portsmouth.

The above notice of the contemplated use of torpedoes in wars is not alluded to in Commander Sleeman's lately-published manual, which, however, mentions the attempt of Lambelli to destroy a bridge across the Scheldt, in the neighbourhood of Antwerp, during the siege of that city in 1585. But all of the foregoing were most probably drifting and floating explodable magazines, not differing in principle much from fire-ships, whereas the essence of torpedo war is subaqueous as opposed to subaërial explosion: it is therefore not until 1775 that we find the present system of submarine warfare dates its inception.

To Capt. Bushnell, of Connecticut, belongs the credit of first inventing the method of igniting charges of gunpowder under water, and he also first devised a submarine boat, with which, however, he failed to destroy the hostile British cruisers.

Next in order of succession come the submarine bombs and carcasses of Robert Fulton, which were tried and rejected by Napoleon; and although encouraged by Mr. Pitt, "England at that time being mistress of the seas, it was clearly her interest to make the world believe that Fulton's schemes were impracticable and absurd." Earl St. Vincent, in a conversation with Fulton, told him in very strong language "that Pitt was a fool for encouraging a mode of warfare which, if successful, would wrest the trident from those who then claimed to bear it, as the sceptre of supremacy on the ocean." Last of all, Fulton submitted to the American Congress his elaborate schemes for rendering American harbours impervious to British attacks. These schemes included drifting, harpoon, and spar torpedoes, together with block-ships, stationary mines, and cable-

* H.M.S. *Vernon*. The Torpedo School-Ship in Portsmouth Harbour.
† H.M.S. *Hecla*. Torpedo ship, now experimenting at Spithead.

cutters; but owing to the unfavourable report of Commodore Rodgers, who Lieut. Sleeman characterises as unfair and prejudiced, Fulton's submarine inventions were not adopted, and the skilled inventor turned his ingenuity to the more humanitarian improvement of the steam-engine.

In fact as long as clockwork, originated by springs, weights, levers, &c., was employed, torpedo science was practically a failure; and until after the introduction of voltaic electricity submarine batteries were regarded merely as futile experimental toys. In 1839 Colonel Pasley, R.E., used galvanic electricity to blow up the wreck of the *Royal George*, and Colonel Colt, whose name will ever be connected with the revolver,* first publicly essayed to explode a case of gunpowder by electricity at a distance, in 1842, and he accomplished the destruction of a vessel under weigh by an insulated electric cable in 1844. Two years subsequently the two great explosive compounds of modern times, viz., *gun-cotton* and *nitro-glycerin*, were discovered; and in the next war that took place stationary buoyant mines under water, fired on contact or observation by means of electricity, were used for defensive purposes by the Russians on a large scale, both in the Baltic and Black Sea, thereby rendering inactive all naval operations on the part of the British squadrons, as much by their moral as by their physical effects. During the Baltic campaign the Russian ports were rendered impregnable against naval attack, if we except distant bombardment, such as Sveaborg underwent, and the capture of Bomarsund, which was practically undefended.

The explosions from which H.M. ships *Merlin* and *Firefly* narrowly escaped deterred the near approach of Admiral Napier's fleet, and his famous signal of "*Lads sharpen your cutlasses*" was thereby rendered a bye-word of bombast.

The breaking out of the Civil War in America brought the science of torpedo warfare more prominently forward, and, as might have been expected, was first used for defence by the Confederates on the Savannah River, causing thereby great delay in the advance of the Federal gun-boats; and in December, 1862, the iron-clad ship *Cairo* was shattered and sunk in twelve minutes by two stationary torpedoes in the Yazoo River—the first instance in history of a vessel of war being thus destroyed in actual war. Subsequently several instances are cited in Commander Sleeman's work

* Colt, when a mere lad, commenced experiments with submarine mines as early as 1829.

of destruction and damage caused by the Confederate torpedoes, but almost all by defensive mines, whereas nearly all the offensive attacks by means of torpedo-boats failed.

Thus there was a total of thirteen iron-clads, monitors, and gun-boats,* besides the aforenamed *Cairo*, all belonging to the Federals, which were sunk by stationary *defensive* submarine mines, without counting others† severely damaged. The only successful *offensive* torpedo attack was fatal to both sides, the Confederate submarine boat being sunk, together with the Federal antagonist, by running into the hole caused by the explosion of her own torpedo.

> "'T is sport to have the engineer
> Hoist with his own petard."
> (*Hamlet*, Act iii., Scene 4.)

As our immortal Shakspeare has it.

On the other hand, the failures of offensive attack were conspicuous,‡ and three Confederate vessels were destroyed by their own mines, owing to the shifting of the position of the barrel torpedoes.

The only Federal torpedo success during the war was that of a boat armed with the Wood and Lay disconnecting-spar torpedo, by which the Confederate iron-clad *Albemarle* was sunk.

Later the *Paraguayans*, in 1866, completely destroyed the Brazilian war-steamer *Rio Janeiro*, by a stationary torpedo, one of the Brazilian fleet which was bombarding Curupaity. This was the end of the second epoch of active warfare in which torpedoes had been engaged, and an interval of over ten years elapsed before submarine warfare was again employed by belligerents; but this time was not idly employed by the submarine engineer, for in 1864 Captain Lupuis, an Austrian, in association with Mr. Robert Whitehead, the Superintendent of Iron-works at Fiume, constructed the "fish torpedo," with which such highly successful experiments were made, that various nations were justified in purchasing the patent on certain conditions advantageous to the patentee; the English Government paying £17,500 for the secret, on the recommendation of a committee of

* Sunk:—Iron-clad, *Cairo*; gunboat, *Baron de Kalb*; transport, *Maple Leaf*; iron ship, *Commodore Jones*; monitor, *Tecumseh*; steamer, *Otsego*; steamer, *Bazeby*; monitor, *Patapsco*; steamer, *Harvest Moon*; and two monitors and three gunboats unnamed.

† Severely damaged:—Monitor, *Montauk*; gunboat, *Commodore Barney*.

‡ Boat-torpedo attacks failed:—Ship, *Ironsides*; ship, *Memphis*; ship, *Minnesota*; frigate, *Wabash*.

officers, who rightly advised the Government not to purchase the *exclusive* secret, for which Mr. Whitehead demanded £54,000, an incident unnoticed by Commander Sleeman, who also passes over in silence the following facts which certainly pertain to the history of this subject. For torpedoes were, alas! not to be confined to civilised belligerents; and as there are in all times and ages uncrupulous individuals who do not hesitate to make war on mankind, so the commercial world was startled by a communication, published in June, 1873, by Mr. H. F. Hemming, Consul for Venezuela, stating that small torpedoes, made to look like lumps of coal, had been actually despatched from France to a Venezuelan port, not named, where a steamer was to be laden with goods of little value, but heavily insured, and this vessel would be sent to sea, having these coal torpedoes on board, in the expectancy that she would be lost and the speculators gain a large sum. A few days later the "Times" published a letter from "Warhawk" on the same subject, and the French Minister of Marine issued a warning circular on the same subject.* Fortunately this timely information put the insurance agents on the *qui vive*, and no explosions or total loss of insured vessels could be traced to this cause. But it does not follow that none have taken place since the scare has been forgotten, and thus it is not out of place to remind the public of such hidden dangers; and the underwriters at Lloyd's should remember that "*Magna pericla latent*" is the apposite motto adopted by the torpedo school-ship, H.M.S. *Vernon*. These *coal torpedoes* or *coal-shells*, models of which were actually exhibited in London (?), were made of gun-metal, and probably contained dynamite with fulminate of mercury so arranged that they would explode in a certain fixed time after being thrown into a steamer's furnace, which would give time for the operator to escape, or, if placed in the coal bunkers, would find their way in due course of time to the fires during the voyage: thus the expenditure incurred in fitting out and loading the insured vessel would be not only recouped, but a handsome sum in insurance gained besides.

In 1875 another species of torpedo was invented, and, like the coal-torpedo above, it was rather sub-aërial than submarine. This was the infernal machine employed by Thomas, and the premature explosion of which, at Bremerhaven, in 1875, caused such destruction,† although its results were

* See "Saturday Review," December 25, 1875, p. 809, from which we extract most of these particulars.

† One hundred and twenty-eight persons killed, besides the wounded, about sixty in number.

inferior to what was expected of it by its suicidal inventor, who, fortunately for humanity, survived his self-inflicted wound long enough to confess his futile machination. This machine of Thomas was not unique, for similar engines were in existence, indeed a *model* of one was obtained in Paris a month before by the gentleman whose knowledge of coal-shells was so intimate, and who by this time has probably perfected with greater accuracy such inartistic inventions. The dynamite in this apparatus was to have been exploded by the concussion of a hammer 30 lbs. weight, set, like an alarum, to fall after a given period--a devilish alarum indeed.

We now come to the latest epoch of torpedo warfare, viz., that inaugurated by that nearly fatal *fiasco*, when H.M.S. *Shah* aimed the first Whitehead torpedo which had ever been fired in earnest against the Peruvian iron-clad *Huascar*, since captured by the Chilians. The attempt was a failure, owing to the latter vessel (fortunately for both parties) altering her course at the instant of discharge. This occurred in May, 1877, and on December 20th, of the same year, "the Russians made an attack with Whitehead torpedoes on an Ottoman squadron lying in the harbour of Batoum; but owing to a want of practical knowledge of the manipulation of such weapons, no vessels were sunk or damaged, but two fish torpedoes—one in perfect condition—were found the next morning high and dry, on the beach at that place." Thus the Turkish Government managed to get the Whitehead secret and torpedo without payment.*

The third and last, and only occasion on which a fish torpedo has been fired successfully against an enemy's ship in actual warfare was on January 26th, 1878, when the Russian steamer *Constantine* fired a Whitehead against a Turkish guard-ship, and completely destroyed her.

From the foregoing we may safely conclude that at present offensive torpedo warfare is most uncertain, the odds being in favour of the defence. But at the same time offensive torpedo operations have hardly yet had their fair trial; the art of torpedo attack is still in its infancy, but its age of puberty is not far distant, and we see the tendency in the numerous newly-invented torpedo-boats, which are ever on the increase, in size, in power, and in speed.

Owing to Commander Sleeman's absence from England, in China and Japan, where he is attempting to induce the Orientals to purchase Capt. McEvoy's lately improved duplex spar-torpedoes, and other patented auxiliaries of

* A lost Whitehead torpedo is advertised for by Capt. Singer, of H.M.S. *Hecla*, since February 5th; an offer of £5 being made for its recovery.

destruction, he has only brought down the history of torpedo-boats to the American inventions of Messrs. Herreshoff, of Rhode Island, U.S.A., whose novelty consisted chiefly in its coil boiler, by which a good working pressure can be obtained within a few minutes of lighting the fire, whilst the fire and the boiler can be blown off in a few seconds. But this advantage has been comparatively nullified by the fact that the Thorneycroft and Yarrow boats can be supplied with steam from the main boilers of the ships to which they are attached. Thus in a late experiment at Stokes Bay a Thorneycroft (No. 56) was supplied with steam from the *Lightning*, beginning to move in 9 minutes 35 seconds, with a pressure of 60 lbs., whilst the *Herreshoff* got under weigh in 8 minutes 2 seconds, with a pressure of 90 lbs.; but after trial on the measured mile the former attained a speed of 14·168 knots, and the latter only 12·972 knots. It was at this trial that the *Herreshoff* boat ran into the *Manly* tug, her sharp nozzle going completely through her side, happily above the water-line; and although the *Herreshoff*'s bow was stove in, owing to her *five* water-tight bulkheads, she floated easily, the breaking of her glass steam-guage being the only damage done to the machinery by the concussion.

Messrs. Yarrow are now building, for the French Government, boats to go backwards and forwards at the rate of 36 knots per two hours,—*i.e.*, an average of 18 knots either way, what is lost in going ahead being gained in going astern. It will be interesting when this guaranteed rate is attained.

The same builders are also building a larger class of torpedo vessels which will supersede the *Lightning* class (first class), measuring 84 feet in length, with a beam of 7 feet 6 inches. The new vessels are to be constructed of a more powerful type, viz., 100 feet in length, with beam of 12 feet 6 inches, and they are to carry enough coal to enable them to steam 1000 miles.

Some boats have also been tried, made by Messrs. Maudslay, Sons, and Field, of plates of manganese bronze, whose thickness does not exceed from one-eighth to one-sixteenth of an inch: they are not quite so stiff as steel plates of the same thickness, and vibrate when the pulsations caused by the propeller are isochronous with those produced by the spring of the boat going at from 10 to 12 knots. This vibratory quivering, however, disappears when a rate of 16 knots is attained.

It is greatly to be hoped that these inventors, in attempting to obtain various improvements in speed and strength

of material, combined with capability of rapid manœuvring, for war purposes, will also serve the ends of peace by promoting novel contrivances adapted to more humanitarian progress.

As Robert Fulton had ultimately to turn his ingenious mechanism from the infernal machine towards the improvement of steam-ships, so we shall soon see all the new improvements of torpedo-boats carried out in more efficiently built vessels for the trade and passenger service of maritime nations. So if the ends justify the means, even extreme "*jingoism*" will have had its share in hastening the progress of modern engineering science.

Since writing the above the news has reached England of the terrible attempt by revolutionary conspirators to destroy the Emperor of Russia in his Winter Palace, on the evening of the 17th of February. General Gourko, who has been charged with the special investigation of this incident, officially reports that, from an examination of the area of destruction and the appearance of the demolition and *débris*, the explosion must have been caused by several pounds of dynamite, ignited in all probability by electricity. The charge appears to have been placed in the cellars of the basement where fuel is stored for the heating apparatus, and this would seem as if the dynamite had been concealed in coal-torpedoes, such as those mentioned in the preceding pages. Although it is thought by some that the mine was sprung prematurely, in accordance with a designed purpose of terrifying the Czar, it seems more probable that an improperly adjusted time-fuze was in reality the cause of the explosion anticipating the instant calculated by the conspirators. It appears that ten lives were sacrificed on the spot, whilst forty-five were wounded, of whom several have since died from the effects, making a total of fifty-five victims on this occasion. It is also suggested that a detonator may have been set in motion by machinery similar to that of the Thomas infernal machine; anyhow the significance of this occurrence is great, and warns us of the presence of secret means of destruction whose origin and design we can only guess at, and in respect to which our knowledge and consequent legislation are alike at fault. It may also serve to remind us of what is yet a very moot question, viz., whether in sanctioning the manufacture of *secret* destructive weapons —nay, in actually encouraging their invention and improvement for purposes of legitimate warfare—we are not thereby unconsciously incurring a certain responsibility as to the use

of these same machines by illegitimate belligerents engaged in internecine war. Lord Vincent's warning at the commencement of the present century is not without point at the present day.

No one can deny that in our time there is an absurd anomaly, which would be ludicrous were it not so terrible, in that civilised nations have, by an international convention, stigmatised the use of an explosive bullet, however openly it may be used (say for the purpose of exploding an ammunition or limber box), because it may mangle or injure a single individual without killing him; and yet these same nations do not hesitate to introduce into their service as an accredited weapon, whose machinery is secret (or at all events presupposed so), a torpedo whose immediate purpose is to utterly annihilate a man-of-war and her crew of nigh a thousand souls. Submarine mines are defensible on all grounds; your antagonist runs his head against them at his own risk; but the offensive torpedo is in the same category as the explosive bullet over which it takes higher rank.

ON WATER AND AIR.*

By JOHN TYNDALL, D.C.L., LL.D., F.R.S.,

Professor of Natural Philosophy at the Royal Institution of Great Britain.

LECTURE III.

I WANT you to be able to associate with every term that I employ in these lectures a perfectly definite meaning. If I speak of things in science, I wish you to be able to present before your minds an image of what I am speaking of. Now, there are various terms that I am afraid I must employ, and therefore it is my duty to try and make clear to you exactly what I mean by those terms.

After a vast amount of experiment and of thought, philosophers have come to the conclusion that all bodies are composed of what we call "smallest parts." An ancient philosopher and poet used to call them "first beginnings." I say that all bodies are composed of these exceedingly small parts—the smallest into which a body can be divided. They are beyond the range of all microscopes, but still we see them with the mind's eye, though they cannot be seen with the eye of the body. These parts are called "atoms." I shall have immediately in this jar a collection of atoms of oxygen gas, which I will pour into the jar from this bottle. The atoms will fill the jar. Mr. Cottrell will open the bottle so as to allow a quantity of the atoms to issue. The jar is, as you see, placed upright, and the gas being slightly heavier than air falls into it, and it is rapidly filled. I will now make it evident to you that I have in the jar the thing which we call oxygen gas. [The jar was filled with oxygen as indicated. A piece of wood was then ignited and plunged into the gas, with the usual result of a strongly intensified combustion.]

If I dip this glowing splinter of wood into the jar, you see that it immediately bursts into flame. The oxygen is capable of producing this combustion.

* Being a Course of Six Lectures adapted to a Juvenile Auditory, delivered at the Royal Institution of Great Britain, Christmas, 1879. Specially reported for "The Journal of Science."

I will now take another jar containing a bundle of atoms of another gas (if I may use the term). This gas is hydrogen. In the case of hydrogen, I take the jar which is to receive the gas and hold it upside down; because the gas, being very much lighter than air, will rise into the jar, like a light body in water, and rapidly fill it.

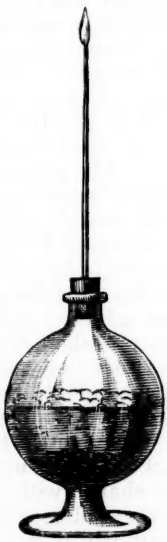
The gas is now entering the jar, and I trust that we shall displace the air in this manner. [The jar having been filled, the hydrogen was ignited.] If I apply a light you will see that I have gathered a combustible gas which burns with the flame which you see before you. If you were close to this jar you would see that it is covered with moisture. What was really done when this flame was produced? In point of fact, the hydrogen burnt in the oxygen of the air. And what was the result of the union of the oxygen with the hydrogen? The result of their union was the production of our familiar substance, water; so that water is composed of these two substances—oxygen and hydrogen, combined together in definite proportions, as chemists say. The atoms of hydrogen and the atoms of oxygen coalesce, and they form little groups of atoms. And here I shall have to use another term of which you ought to know the meaning. The manner in which these things combine is this: two hydrogen atoms single out one oxygen atom, and unite with it, so that the smallest particle of water is composed of a group of three atoms, two of which are hydrogen and one of which is oxygen. This group of atoms has a different name from the atoms themselves. It is called a "molecule." Hence we have two "atoms" of hydrogen and one "atom" of oxygen, and we have one "molecule" of water produced by their union. We can take water and produce from it these gases very easily indeed. The ordinary way of producing this hydrogen gas is to pour a little dilute sulphuric acid upon zinc. Here are fragments of zinc, and here is sulphuric acid diluted with water. The zinc has, as chemists say, a power of attraction which it exerts upon oxygen, and when this liquid is poured upon zinc, the zinc seizes upon the oxggen and allows the hydrogen to go free. I will now pour into this globular vessel, containing fragments of zinc (Fig. 16), some dilute sulphuric acid, and you see we get a stream of hydrogen gas issuing from the liquid.

By applying a light to the end of the jet attached to the apparatus the issuing gas can be ignited, and it burns with a pale, lambent flame.

In this case we use granulated zinc; that is, zinc broken up into small pieces, for the purpose of enabling the acid

more easily to act upon the metal. Other metals will act in the same way. For instance, here I have a curious metal which was discovered in this place by the famous Sir Humphry Davy. It is a metal called potassium, which is

FIG. 16.



extracted from ordinary potash, and the attraction of this metal for oxygen is so powerful that, no doubt, we shall be able to burn the metal even upon ice: for ice, as you know, is solid water. If we place a little piece of this metal upon the ice, you see that it actually burns upon the ice. How is it that it burns? It burns because it seizes upon the oxygen of the water and allows the hydrogen to go free; and that purple flame which is produced is due to the liberated hydrogen which has been ignited by the heat produced by the chemical union between the potassium and the oxygen.

Thereis another thing with which I should like, at all events, to begin to familiarise your minds, and that is the nature of what we call heat. You saw it developed here by this combination. It is a notion universally accepted at the present day that this thing which we call heat is a brisk motion or vibration—a quivering, so to say—a tremor of the

atoms and molecules of bodies. I have explained to you already what I mean by atoms and molecules. As I said, this opinion is universally entertained at the present day; but it is curious to observe, in the course of history, how this opinion has developed itself, and how early the minds of sagacious, penetrating men saw and seized upon this idea that heat was what we call atomic or molecular motion. At the time of Sir Isaac Newton there were men who seized this idea that heat was a motion; but there was, in particular, one man of the greatest genius, named Robert Hooke, who endeavoured to give his mind a definite image to rest upon with regard to what we call fluidity—that condition which we get when this substance, ice, is converted into liquid water. Hooke's image is so quaint and so penetrative that I think I will read his account of his conception in his own words. He says:—"First, what is the cause of fluidness? This I conceive to be nothing else but a certain pulse or shake of heat: for heat being nothing else but a very brisk and vehement agitation of the parts of a body (as I have elsewhere made probable), the parts of a body are thereby made so loose from one another, that they easily move any way and become fluid." So with the particles of our ice when they are converted into water. He continues: "That I may explain this a little by a gross similitude, let us suppose a dish of sand set upon some body that is very much agitated and shaken with some quick and strong vibrating motion, as on a millstone turned round upon the under-stone very violently whilst it is empty, or on a very stiff drum-head which is vehemently or very nimbly beaten with the drumsticks. By this means the sand in the dish, which before lay like a dull and inactive body, becomes a perfect fluid, and ye can no sooner make a hole in it with your finger but it is immediately filled up again, and the upper surface of it levell'd. Nor can you bury a light body, as a piece of cork, under it, but it presently emerges or swims as 'twere on the top; nor can you lay a heavier on the top of it, as a piece of lead, but it is immediately buried in sand, and (as 'twere) sinks to the bottom. Nor can you make a hole in the side of the dish but the sand shall run out of it to a level. Not an obvious property of a fluid body, as such, but this does imitate; and all this meerly caused by the vehement agitation of the containing vessel, for, by this means, each sand becomes to have a vibrating or dancing motion, so as no other heavier body can rest on it unless sustain'd by some other on either side: nor will it suffer any body to be beneath it, unless it be a heavier than

itself." You see the image before his mind; and, in teaching the young, I always like to keep before them certain definite, clear images that will answer to the terms which we employ. Hooke's image of the vibrating sand particles may, perhaps, help you to conceive of the vibration of the particles of water which takes place when ice is converted into a liquid. Well, Hooke's image of fluidity answers very well for our conception of the mere liquid state. But now suppose that you have a liquid such as I have here, with a free surface—with those molecules and atoms vibrating in this way. I have now to appeal to the eye of the mind. I can not show these things to the eyes of your body, but I am sure that every boy here present who will give me his attention will be able to see mentally what I mean. Just as in the case of the Whirlpool Rapids which I showed you in the last lecture, where two waves coalesced and tossed the crest of the wave high into the air, so you can imagine the vibrations sometimes so coalescing at the free surface of the liquid that they shall toss the molecules of the water into the air. If you have seized this image, you have now what we call evaporation. The particles of liquid are loosened by the vibrations produced by heat, and they are incessantly jerked into the atmosphere above the liquid; and therein consists all evaporation which will reduce the whole of the liquid to vapour if you only allow it sufficient time. This, then, is our conception of evaporation.

Now I approach the subject with which we started—the conversion of the waters of the ocean into vapour, that vapour from which all our rains and rivers are derived. You may say to me, "The water of the ocean is salt, and the water coming from the clouds is perfectly sweet and without salt. How can you get the sweet water from the salt water?" Quite easily, I answer. There, for instance, is a flask (F, Fig. 17) containing a very strong brine—so strong that after boiling it for a time, by means of a gas-flame, a large quantity of the salt is rendered solid in the bottom of the vessel. We kept that boiling yesterday for a considerable time. We led the steam from that flask through the worm-pipe, placed in the glass vessel C, that you see there. We have surrounded the worm with cold water, kept cold by ice. The steam passes over from that flask of salt water, and, after passing through the cool worm-pipe, it is received in the form of water in this flask, F′. There is the fresh water extracted from the brine. This water is perfectly sweet—sweet as any spring-water. This shows you that in the process of distillation, whether it is artificial or is pro-

FIG. 17.

FIG. 18.

duced by the sun, you liberate not the molecules of the salt, but the molecules of the water only, and thus from the salt ocean you get sweet water.

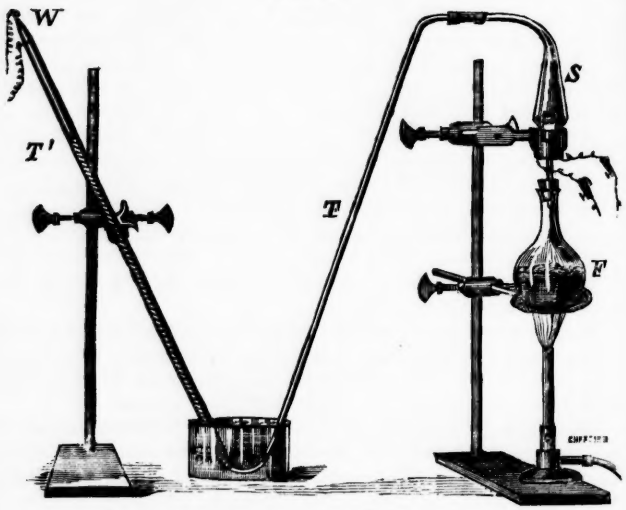
You must, at the same time, have perfectly definite ideas of the meaning of vapour. Here is a boiler, B (Fig. 18), and beneath it we will now start a flame so as to make the boiling of the water go on vigorously; and there you have, issuing from the jet J,—what? Will you call it steam, or will you call it vapour? If you called that cloud either steam or vapour you would be in error. That cloud that you see issuing from the boiler is not vapour. It is composed of fine particles of water. It is, as I have elsewhere expressed it, a kind of dust of water,—particles of water finely divided and precipitated, so as to produce this appearance. But if I take a flame and bring it underneath the cloud, at a short distance from the jet, the cloud entirely disappears. I can cut it off in this way, at the very nozzle of the tap, and no cloud whatever is seen. Now that invisible thing into which I have converted the visible cloud is true vapour. The vapour of water is as invisible as the air that you breathe. This cloud that you see is the vapour of water precipitated so as actually to form water; and in that way our clouds are produced.

Now I want your patience to accompany me while I make a thorough investigation of the manner in which this vapour is raised from the ocean—an investigation of the agent which is instrumental in producing this vapour.

Before I do that, however, here is an experiment which my friend Prof. Dewar has arranged for me, and which enables me to follow up the idea which I threw out before you—that heat consists of this intense vibratory motion to which I have referred. I mentioned in our last lecture the name of Sir William Grove,—Mr. Justice Grove,—and I told you how he had operated upon boiling water. He made a series of extremely interesting experiments many years ago upon the action of heat upon water. He placed pieces of intensely incandescent platinum, or platinum wire, in water, and he found that the atomic vibrations of which the intense heat consisted were so violent that they actually shook asunder the molecules of water, and reduced them to their constituent atoms of oxygen and nitrogen. Here we have an apparatus which will show you this action (Fig. 19). At the present moment Prof. Dewar is going to ignite, by means of electricity, the spiral of platinum wire contained in the glass tube S, by attaching a battery to its terminal wires. Observe what is going on. The water is boiling vigorously

in the flask F, and steam is passing over this incandescent spiral. The mind of every boy here present must figure the atoms of the spiral as vibrating in the most intense manner and coming into collision with the molecules of steam which are passing over the spiral. You must imagine that the vibrations of the incandescent spiral are shaking those molecules of water asunder, and are converting the water into its

FIG. 19.

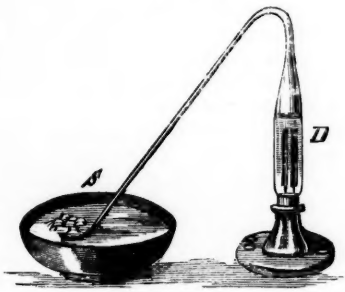


constituent atoms of oxygen and hydrogen. You see those bubbles that are being carried down the tube T, and are rising in the tube T′ and collecting in the upper part of the tube. These bubbles that you see rising are not bubbles of steam, and they are not bubbles of air, but they are bubbles of the mixed gases, oxygen and hydrogen, which are the constituents of water. Here the gases are collecting, and if we allow the operation to go on for a minute or so we shall obtain a certain amount of gas at the upper part of the tube. We can cause the two mixed gases to combine immediately, simply by sending an electric spark between the two points of platinum which are sealed into the top of the tube, and you see the bright flash of light produced at the moment of their combination. Thus, by this simple experiment, we obtain, mechanically speaking, a shaking asunder of the

molecules of water and a reduction of the water to its constituent parts. I could decompose water and reduce it to its constituent elements in another way,—by electricity,—but in this case it is simply done by heat.

Now I want to show you the decomposition of water in another way. I have here an apparatus (Fig. 20) by means of which I can send an electric current through the water, and cause it to be resolved into its constituents oxygen and hydrogen. In the glass vessel D are two platinum plates, which are completely surrounded by water. The upper part of the glass vessel is contracted into a narrow tube, S, which

FIG. 20.



is bent over, and the end immersed in a solution of soap-suds contained in a basin. Here is one end, or, as we call it, one pole of a voltaic battery; and here is the other pole. If we now connect this battery to the platinum plates contained in the vessel D, we immediately obtain a stream of gas rising from the plates, and passing over through the tube S, collect in bubbles on the surface of the solution of soap-suds.

We will take some of these bubbles on the hand, and see whether they do not contain a mixture of oxygen and hydrogen. After a short time all the air that was in the apparatus in the first instance is entirely expelled, and we have nothing but the mixed gases issuing from the apparatus. [A few bubbles of the mixed gases having been collected on the solution of soap, a light was applied to them, and they exploded with a loud detonation.] You maytake these bubbles on your hand and explode them without any evil effect whatever. But you have just heard the noise which accompanies the combination of the oxygen and the hydrogen.

We will now pass on to the consideration of other things. I trust that you have a clear conception of what is meant by evaporation and by vapour. I want to proceed to the investigation of the particular agent which produces the aqueous vapour of our air, and which produces all our rains, snow, and hail. Of course I need not tell you that the agent that raises the vapour from the surface of the sea is the sun. The sun pours down his heat upon the sea and warms the water, and this water rises into the air as vapour, and when it is condensed in the air it falls as rain.

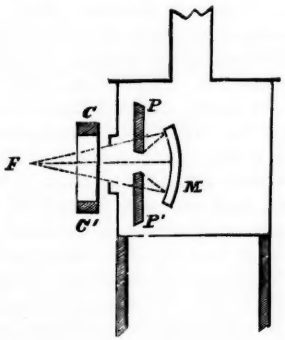
The investigation which I propose to place before you at the present moment is this:—How does the sun bring this about? Are the whole of the sun's rays influential in producing this evaporation? Are those rays which enable us to see bodies influential in producing it? Are those rays that produce vision influential in evaporating the water of the ocean? All these questions we have now to examine; and I trust that you will give me your patience if I try to examine them to the very bottom.

In the first instance I will try to produce before you what I will call an artificial sun. [A powerful electric current from a magneto-machine was set in action, producing an intense light and heat.] This, then, is a little artificial sun with which we are going to operate, and that it may not annoy you too much we have enclosed the lamp which regulates this current in this kind of camera. This light is produced by a gas-engine downstairs, which works a beautiful machine which has been kindly presented to the Institution by Mr. William Siemens. The rays from this little sun shall impinge upon a reflector such as I hold in my hand. They will be gathered up by the reflector and thrown forward. Here is our camera (Fig. 21), and I will place within it this reflector M, and through the aperture in front of the camera we will reflect the rays emanating from the intensely-heated carbon-poles P and P′, and there I obtain an intense focus of heat and light at F. Now I want to show you that that beam is capable of producing ignition. For instance, I hold this piece of paper at the focus F. [The paper immediately took fire.] Look at that! That is done by the pure radiation—not by any flame. The paper is ignited by the condensed radiation from our little domestic sun. Here is a metal, and I dare say that this metal will burn just like the sheet of paper, if I place it in the focus of the light. [A piece of thin sheet zinc, upon being held at the focus, immediately burnt with its characteristic greenish blue flame.] We have the zinc burning like so much com-

mon paper. The heat is so intense that I could have readily ignited a diamond. A very famous experiment of the celebrated Florentine academicians was to ignite a diamond by means of a tremendous heat. Another way of converging the beam is by means of what is called a conjugate mirror. I will send a wide beam out through the aperture in our camera, and will then receive that beam upon another mirror. The second mirror will gather up the beam to a point or focus, and at that point we shall obtain all these tremendous effects to which I have been referring. I obtain in front of the lamp a fairly parallel beam, which, in the first instance, is not collected to a point or focus. But it is collected to a focus by the second mirror. [A focus having been produced, pieces of paper and sheet zinc were ignited therein, as in the former experiment.]

I now want you to observe a further experiment. I have here a substance which, practically, does not interfere at all with the passage of the light which emanates from the

FIG. 21.



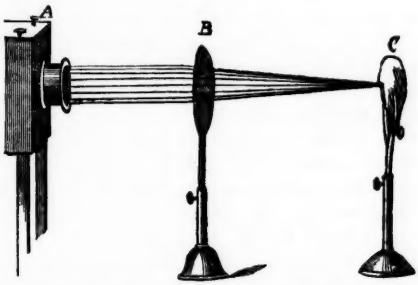
carbon-points. The light goes through it with the greatest ease. [A flat glass cell (c, c′, Fig. 21), containing a solution of alum in water, was interposed in the track of the converging rays, in such a position that the focus fell beyond the cell.] I now place in the focus a piece of clean white gun-cotton. You see that the gun-cotton remains perfectly unignited, although it is a very inflammable substance. Now we will remove the cell containing the solution of alum, and you will at once notice what occurs. [The solution was removed, and the gun-cotton instantly exploded.] The solution of alum, being a perfectly transparent thing, does not

interfere with the luminous rays coming from our little sun. It must have been something else than the luminous rays which caused that gun-cotton to ignite. But now let us see whether these luminous rays do not possess heat. You will find that they do, for if I darken the gun-cotton with a black powder you will find that the luminous rays are quite competent to ignite it. [The cell of alum water was replaced in the track of the beam.] We will place our blackened gun-cotton in the focus, just as in the former case; and you will see that, although this beam of light had no effect whatever upon the white gun-cotton, yet when the gun-cotton is blackened you have an explosion, although the light has passed through the solution of alum.

Now this leads me to say a few words for the purpose of inquiring into the cause of these differences; and I think that if you follow me you will understand it perfectly well. I will tell you an experiment that I made yesterday. That is an exquisite plate of glass of the purest materials prepared for me by Mr. Chance, of Birmingham. It is perfectly white glass, altogether uncoloured, and you would imagine that it would be perfectly transparent to the radiation from our electric light. Here is another plate [a plate of rock salt] not more transparent than the plate of glass, if looked at by the eye. Yesterday we placed it in the path of the beam. What occurred? First of all, the glass plate was placed in front of the lamp, and we examined the beam that had passed through it. We found it enfeebled by its passage through the glass plate. We placed in the same position this other plate, which consists of rock salt, which is perfectly transparent, and we also examined the beam that passed through the rock salt. We found the beam much less enfeebled than it was by this transparent glass plate. I then felt the rock salt by placing my hand upon it, and it was almost perfectly cold. I touched the glass plate, and I almost burnt my hand; so that the glass had the power of intercepting the heat and lodging that heat in the body of the glass, which power was not possessed by the rock salt. In scientific language, we say that this glass is a better absorber of heat than the rock salt. The rock salt transmits the heat and does not absorb it: the glass absorbs the heat and does not transmit it. The effect of this difference is that a body that does not absorb heat can not be heated. A perfectly transparent body, when it is operated upon by the rays of pure light, can not be heated by those rays of light. For instance, if I place my alum cell in front of the lamp and allow the heat to pass through it, I might place

behind that alum cell a sheet of paper, a sheet of glass, or a sheet of ice, sensitive as this substance is, and the rays that pass through the alum cell would be perfectly incompetent to warm the water or to melt the ice; so that these luminous rays are perfectly incompetent to heat transparent bodies. But if a body can absorb the luminous rays, then those rays can make themselves manifest, or the action of the rays can make itself manifest. And hence it is that when I operated upon clean gun-cotton, the fibres of the gun-cotton, being transparent, did not at all absorb the luminous rays; and when I converged upon it simply the luminous rays from our lamp those rays had no power to explode it. But when I allowed the total radiation from the lamp to fall upon it, then I exploded it. And also when I blackened the gun-cotton, those luminous rays were competent to explode it. You might suppose that rays that had passed through water or through ice had no power to heat anything afterwards.

FIG. 22.



You might imagine that in passing through cold water or ice the beam would be entirely robbed of its heat; but I want to show you that that is a delusion. Here, for instance, I will take a piece of blackened gun-cotton as before, and I hope to be able to show you a very beautiful lens of ice. There it is—B (Fig. 22)—a lens of perfectly transparent ice. I intend to make use of that ice as a burning glass. The luminous rays shall pass from our camera, A, through the ice, and they shall be converged by the lens, B, and I expect that at the focus of the lens we shall be able to explode the gun-cotton, C. Inasmuch as these are luminous rays, the gun-cotton is blackened in order to enable it to absorb them. We will, first of all, introduce a cell of water between the camera and the ice lens. You see that here we have a beautiful parallel beam, and Mr. Cottrell will interpose the ice

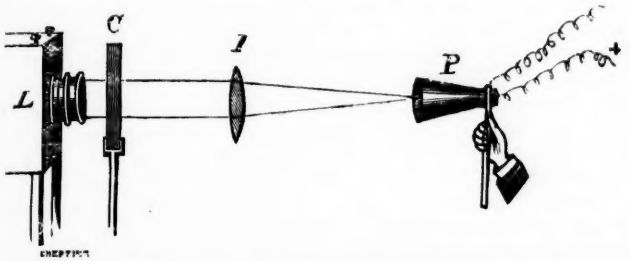
lens in the path of that parallel beam. Those persons in front will observe the convergence of the rays by the ice lens. The lens brings the rays to a focus, and I will now remove our cell of water and there we have instantly the explosion of our gun-cotton.

Before coming into the room, I thought that I would try and hasten the next experiment [that of making water boil by means of the beam from the electric lamp] by heating in a test glass the water that I am going to use before placing it in the focus; but I think that it will be better and fairer for the experiment if I begin with cold water, operating with that cold water as the sun operates with the cold water of the ocean. Mr. Cottrell will now give me a little cold water from the tap, and I will converge the beam so as to produce an intense focus in front of the lamp. I will now bring this cold water in a glass tube in front of that focus, and we will place across the beam the cell containing the solution of alum. The cold water may remain in the focus till doomsday without the light producing any sensible heat in the water. The beam of light having passed through the cell of alum water, all the rays that are capable of heating water are completely taken away from the beam; so that I might hold the water in a focus for any length of time without its even becoming warm. We will now take away the cell of alum and water, and we shall soon see that the water in the tube will be raised to ebullition. In order to make the experiment as conclusive as possible, it is worth while to begin with cold water. In a very short time, if you give me your patience, I trust to be able to boil the water in your presence. At the present time I see bubbles of steam being generated, and after a little while you will find it visibly boiling before you. I hear it beginning to boil already. I now feel the dancing of the water. [After a short interval.] Now I think that those in front can observe that the water is actually boiled by radiant heat. Mr. Cottrell will now introduce a cell containing alum water. The boiling instantly ceases. The light is no longer capable of boiling the water. We take the cell away, and the boiling recommences immediately. Now I will ask him to interpose a substance which will entirely cut away the light, and you will find that the ebullition of the water will not cease. In fact, to speak particularly, the emanation from this artificial sun of ours, just like that from the natural sun, consists of two classes of rays, one capable of exciting vision, and the other, far more powerful, capable of exciting heat and not vision; and here, in this case, the alum cell or the water cell cuts off a

great portion of these rays that are not capable of exciting vision. But you might infer from the experiment that we have made that there is something besides the light-giving rays that produced the effects that you have seen—the burning of the paper and the exploding of the gun-cotton.

Now we want to detach from the electric light this particular portion of the emanation that is effective in producing those combustions and in boiling the water; and for this purpose we must discover a substance which shall cut off all the luminous rays, and allow to pass only those rays that are called invisible rays. Here I have such a substance. Mr. Cottrell has here a cell containing it. He will throw upon the screen the image of the carbon points from which our light is produced, and here is the substance to which I have referred. It is perfectly dark. If you look through it at the sun at noon-day you will find that this substance will cut off the rays of light. You will see that the opaque liquid contained in this cell will completely cut off every

FIG. 23.



trace of the light from our little domestic sun. No trace of that light can pass through the liquid. Now what I want you to understand is that through this opaque solution a certain portion of the radiation from our domestic sun will pass absolutely with perfect freedom, and we shall be able to cause water to boil in the focus. When that has been done, I will interpose the alum cell in the path of the beam instead of the opaque solution, and you will see that when the alum cell is used the boiling immediately ceases. You will see that although all the light has been cut off by the solution, the water will still continue to boil. I said that we might have inferred from our former experiments that there was in the beam, besides the light, something which caused the water to boil; but inferences ought never to be accepted in science if we have an opportunity of converting

those inferences into facts. Perhaps it is worth while to make an experiment. By means of this larger lens—I (Fig. 23)—I will converge upon the beam emanating from the camera L the dust of the room. You see the beam passing through the air of the room. If we bring the lens forward a little from the light, you have a conical beam, and here you have the point of convergence of the beam at P. I will just mark that point of convergence. I have here an instrument (held in the hand at P) which will enable me to test whether or not there is any heat in that beam. Mr. Cottrell has associated this instrument with the magnetic needle which you see suspended there before you; and if I breathe for a moment on this instrument, you will see the effect which the warmth of my breath produces upon the needle. It is an instrument used for the purpose of converting heat into electricity, and causing the magnetic needle to move. The warmth of my breath will be sufficient to produce a large deflection of the needle. I breathe, and there you see the needle moves away, and, as I have said, a large deflection is produced.

Well, now, here we have our beam of light converged in the dust of the room. I will place upon this side of the instrument [the thermo-electric pile P] this cap, and on the other face I will place a little reflector. It will require a little time before the effect of the waste heat that I communicated by my breath disappears. We will now bring the needle to rest. We will cause the pile to get cool by presenting it to a cool part of the room, and the needle will gradually come back to zero. If I present the pile towards the piece of ice, perhaps it will be still better. It will cause the needle to come back still more rapidly towards zero. If I place this instrument in the path of this beam of light, you will get a deflection of the red end of the needle towards me. Now I want you to see that if we place this opaque solution (contained in a cell, C,) in the path of the beam and entirely cut off the light, still the heat will pass through, and fall upon the pile, and you see that effect produced which is indicated by the movement of the needle.

Now I want to collect these heat rays, and show you that they are capable of boiling water. We have here our focus (F, Fig. 21), and I will boil the water as before. Mr. Cottrell will go to the other lamp and cause its light to fall upon the boiling water, for I want you to see the experiment. [The tube of water which had been used in the previous experiments was again caused to boil in the electric beam. The cell (C, C′, Fig. 21) containing the opaque solution was

then interposed. The opaque liquid consisted of a solution of iodine in bisulphide of carbon.] You saw a moment ago that the interposition of the alum cell completely stopped the boiling; but now, when we put in this cell of opaque liquid, the boiling continues as before. So here we are boiling the water by the invisible rays and not by the visible rays, and this is the point that I am labouring to bring you to. I say it advisedly, for it has been some labour. I have been labouring to bring you up to this point—that it is not the luminous rays of the sun that cause the ocean to evaporate and that cause the vapour to go into the atmosphere, but that it is the invisible heat-rays of the sun that produce this effect. If I wished to show you various experiments in connection with these non-luminous rays, I could show you that they have power to produce all the effects of ignition and combustion which we have already produced at the focus of the full beam. Just allow me to make one such experiment. Mr. Cottrell will give me a beam. Here is a focus produced in the air by the reflection of the rays. If now I take a piece of black paper, and hold it in the focus of the rays, you see that it instantly bursts into a blaze. We will now take the opaque solution which will destroy all the light, and place it in the beam. I now interpose a piece of black platinum, and hold it in the perfectly dark focus of the rays, where there is no luminosity whatever, and you find that the platinum is raised to vivid incandescence. So if I place a piece of blackened zinc in the same position, it immediately bursts into flame. Again we place our blackened paper in this invisible focus, and it is immediately set on fire. You see the paper bursts into flame at this focus, which is entirely without light.

And here we come to the conclusion, with regard to the origin of our rains, and hails, and snows, that all these are due to the sun, and that they are mainly and almost exclusively due to that portion of the sun's rays which does not at all administer to vision—the invisible rays of the sun of which you would not have known except by means of the experiments which we have made, and which were first made by the celebrated Sir William Herschel.

EXPLANATORY REPORTS ON NEW SCIENTIFIC PROCESSES AND INVENTIONS.

[These Reports will not partake of the nature of an Advertisement, but will be impartial descriptions.—Ed. J. S.]

Applications of Artificial Cold in Industrial Chemistry.

No fact is more familiar to scientific men than that the behaviour of substances brought in contact is powerfully modified by the temperature to which they are submitted, the results produced being affected not merely in degree, but sometimes even in kind. But whilst in manufacturing operations temperatures ranging from the average heat of the atmosphere up to the melting-point of platinum have stood at the disposal of the technologist, and have been applied as requisite, it is only of late that what is familiarly known as *cold—i.e.*, temperatures below the freezing-point of water—has been made available on a practical scale.

The following examples will at least show that the freezing-machine, as the antipodes of the furnace, has a future before it of which inventors will do well to take heed :—

M. Georges Fournier, of Paris, has devised a method for obtaining sodium sulphate, commonly known as Glauber's salt, depending on this novel industrial agency :—In various parts of France, especially in the neighbourhood of Rheims and in Picardy, there occur extensive deposits of pyritic shales far too poor in sulphur for use in the kilns of sulphuric acid works, and only occasionally utilised in the manufacture of copperas and of alum. But now the latter product is generally prepared from china clay, or from bauxite, and now the works at La Tolfa have been revived by means of French capital, and are sending to France and elsewhere large quantities of the much esteemed "Roman alum," the makers of alum from pyritic shales—despite the cheapness of their raw material—find themselves driven out of the market.

The problem, then, which M. Fournier undertook to solve is this :—How are such shales to be utilised, and especially how can the sulphur which they contain be made to effect the decomposition of common salt and the production of sodium sulphate ?

He takes the shales in the state in which they were formerly used by the alum and copperas makers. On prolonged exposure to air, namely, the sulphur of the pyrites becomes oxidised to sulphuric acid, and, attacking both the iron and the clay with which it is intimately associated, it forms a mixture of iron and aluminium sulphates. The shales are then lixiviated with water, and as the solution generally contains an excess of acid it is neutralised, as far as possible, by being allowed to stand in contact with scrap-iron. Being then run off into crystallising tanks a very considerable quantity of copperas is deposited. So far, it will be clearly understood, M. Fournier is merely following in the footsteps of the manufacturers of copperas and alum. But now comes the new feature of the process. After the copperas has been deposited, the mother-liquor, containing all the aluminium sulphate and a portion of iron sulphate, is run off, and mixed with common salt in such proportions that there may be sodium enough to combine with all the sulphuric acid, and chlorine enough to take up all the aluminium and the iron. The mixed solution, which should not exceed 23° Baumé in specific gravity, is then exposed to a temperature of – 2° to 3° C. In what manner this temperature is produced is a matter of indifference as far as the principle of the invention is concerned. When I saw the process worked on the large scale at the establishment of M. Raoul Pictet, in La Chapelle, the sulphurous acid freezing-machine was used with very good results. In a short time double decomposition takes place, and we have in solution—instead of sodium chloride and aluminium sulphate—sodium sulphate and aluminium chloride. As, however, at temperatures bordering on and a little below 0° C., sodium sulphate is almost insoluble, it is deposited in the state of a fine crystalline sediment of the deca-hydrated salt (the ordinary form of Glauber's salt), whilst the aluminium and iron remain in solution as chlorides. The mother-liquor is rapidly run off, and the deposit is freed from the chlorides mechanically lodged among its crystals by washing with brine cooled down to 0° C. by an ingenious application of what might be called the waste cold. This brine afterwards serves towards the decomposition of the next lot of sulphates of aluminium and iron, so that nothing is lost. The crystals after washing are freed from moisture in a centrifugal machine, and are then fit for any purpose to which Glauber's salt is applicable. I have lately learnt from the inventor that he has succeeded, at a very trifling cost, in using this salt as a raw material for the production of caustic soda.

We return now to the mother-liquor—a solution of chlorides of aluminium and iron. One of the guiding principles of industrial chemistry is that every result of a decomposition should be brought out in a merchantable form. How far can this be carried out in the present instance? To obtain chloride of aluminium in a hydrated state a very costly and circuitous process has

hitherto been employed. The commercial sulphate of alumina —so called cake-alum—is dissolved, precipitated with an alkaline carbonate, and the precipitate re-dissolved in hydrochloric acid. Hence its price has naturally been greater than that of the sulphate, and the latter salt has therefore been employed in preference. There is, however, one class of purposes for which the chloride of aluminium would at equal prices be preferred to the sulphate. Several years back it was very justly remarked by Mr. Crookes that the settlement of the sewage question must to a great extent turn upon cheap soluble alumina. The power of alumina in such states to seize upon organic matter not merely when suspended, but when dissolved in water, and to precipitate it in combinations of which "lake-colours" are the most familiar type, is well known to practical men. How many of the every-day operations of the dyer and the colour-maker entirely or mainly depend upon this very power I need not stay to point out. That the action of aluminous salts upon fœcal matter is closely analogous to their behaviour with tinctorial principles must be admitted when we see that, according to the recent official investigations of Dr. R. Angus Smith, the "albuminoid ammonia," or organic nitrogen, in the effluent waters from the Aylesbury Sewage-works, was found as low as 0·024 grain per gallon,—a little more than one-fifth of the limit proposed as admissible by the late Royal Rivers Pollution Commission.

Yet so long as the soluble salts of alumina were high in price results such as the above, however interesting from a theoretical point of view, were of little value to the practical sanitarian. Soluble alumina, in the form of sulphate, has indeed experienced a remarkable reduction in price during the last eight years; but, thanks to M. Fournier, its future cost in the state of chloride will be even lower than it is at present. Nor should it be overlooked that, other things being equal, chlorides are decidedly preferable to sulphates for sanitary purposes.

The aluminium chloride prepared as described above is not applicable to dyeing and to the destruction of vegetable matter mingled with wool, on account of its contamination with iron, an impurity which in the treatment of sewage and night-soil is by no means objectionable. But for the preparation of a pure aluminium chloride M. Fournier proposes to use the ordinary cake-alum as a starting-point. This salt is dissolved, mixed with a proper proportion of brine, and refrigerated as described above. The results are Glauber's salts and aluminium chloride fit for the so-called "carbonisation" of wool, and of course cheaper than that obtained in the ordinary manner.

Another French inventor, M. Pechiney, of Salindre, in order to utilise the "sel mixte" of the salt-works on the Mediterranean coast, a mixture of common salt and of magnesium sulphate, exposes the lyes by means of Carré's ice-machine, to a temperature of $-6°$ C. The results are analogous to those obtained in

the Fournier process; Glauber's salt is deposited in a fine crystalline mass, whilst chloride of magnesium—an article now largely consumed for weighting textile fabrics—remains in solution.

M. Pechiney proposes a very ingenious process for converting this hydrated sodium sulphate into the anhydrous salt, known in the trade as salt-cake. About 1500 kilos. of the crystalline paste, previously drained, are placed in a wooden cistern fitted with an agitator, and dissolved in a little of the mother-liquor with the aid of steam introduced by a copper worm. When the whole is dissolved, 250 kilos. of common salt are added. An exchange now takes place; the salt gradually dissolves, and the sulphate is deposited in its stead, but in an anhydrous condition. This sediment is then transferred to a filtering-vat with a double perforated bottom, and allowed to drain. It is thus obtained free from all foreign matter, save about 5 per cent of water and 0·5 of common salt.

It is highly probable that the processes here described may point the way to further applications of low temperatures in the chemical arts.

J. W. Slater.

ANALYSES OF BOOKS.

*Allgemeine Ethnographie.** Von Friedrich Müller. Wien: Alfred Hölder.

We have here a luminous, compact, and yet comprehensive survey of an important science, furnished by a masterly hand. In the outset the author points out the distinction between ethnography (ethnology) and anthropology. The latter science regards man, physically and psychically, as a specimen of the zoological species *Homo*. The former views him as an individual belonging to a certain society, depending on custom and descent and united by a common language. After sketching the history of the science and explaining the reason of its absence in classical antiquity, he explains the meaning of the terms "race" and "people," which may be summed up in the words that race is an anthropological, but people an ethnographical, conception. He remarks that though we now find no men not belonging to some people, we must still admit that there were at one time no peoples, but merely races.

Proceeding to the systems of anthropology and ethnography, he gives a summary of the classifications of Blumenbach, Cuvier, Retzius, and Haeckel, the latter of which, mainly founded upon the properties of the hair, is expounded at some length. According to this arrangement man is primarily divided into Ulotriches and Lissotriches. In the former division each hair is a flattened band, having an ovoid section. In the Lissotriches the hairs are cylindrical, and their section is a circle. To language the author—in our opinion very judiciously—assigns merely a subordinate value in the diagnosis of peoples.

We next meet with the question, What is the position—we might say the valence—of human races in zoological systematics? Herr Müller, in acccord with Darwin, regards them as sub-species. We must here, however, point out that the mutual fertility of different human races can scarcely be taken as a ground for denying them the rank of distinct species, when we find the American bison and the domestic cow—though belonging to separate genera—still capable not merely of interbreeding, but of producing a permanently fruitful progeny. The various races he considers have arisen by natural selection from one primitive type, which has long since disappeared, and which branched out first into the Ulotriches and Lissotriches.

* Universal Ethnography.

As regards the origin of man the author declares himself an Evolutionist, whilst fully admitting that neither of the existing theories of the origin of animal life can be regarded as free from difficulties. The existence of mankind upon the earth he traces back to dates far earlier than those of the vulgar chronology. The dawn of Egyptian culture may be placed about B.C. 5500, whilst a "Mediterranean race" probably existed as far back as 9000 to 10,000 years B.C. The original home of man he seeks in the warmer regions of the eastern hemisphere. We may particularly notice here the following passage:—"Another circumstance which points to a warm climate is the general nakedness of man, which cannot be explained by the subsequent use of clothing, since savages who go about naked, far from being more densely haired, are, with few exceptions, characterised by the baldness of those parts which in civilised man are covered with hair." The author here seems to overlook the fact that in one of the lowest existing types of mankind, the Australian aborigines, the back is clothed with dense hair.

The question whether or no mankind is descended from an original pair, Herr Müller justly sets aside as totally unscientific.

The author next points out the peculiarities, physical and psychical, in which man differs from the rest of the animal world. He next examines the distinction between the various human races, and insists strongly upon their permanence. On the oldest Egyptian monuments the negro is depicted as we find him to-day, an interval of from 4000 to 5000 years having failed to modify his structural characteristics. We may here remark that in spite of this persistency the advocates of mechanical creation consider him as of one species with the European, and as descended from the same first parents. Yet with scant consistency they take a similar permanence, as seen in the domestic animals of ancient Egypt as a proof of their original and permanent distinctness and of the invariability of species! The mixture of races of approximately equal rank yields, he considers, an improved offspring. The contact of very unequal races is, however, fatal to the weaker.

In succeeding chapters we find an examination of the influence of soil and climate upon human culture, an account of the ancient centres of civilisation, of its conditions, and of the part played by the different human races in its development, where the lowest rank is assigned to the Australian. The wanderings of nations are not investigated. It is remarked that very few tribes can be rationally considered as aborigines of the countries which they now occupy. The Caffres and Zulus, for instance, have entered South Africa as intruders from distant regions in the north-east, and their rights are simply those of conquest—a fact which our professional philanthropists would do well to consider. The original seat of the Indo-Germanic (Aryan) race the author

seems to seek in South-eastern Europe. As an argument in favour of this view, he contends that the linguistic remnants common to all the subdivisions of the Aryan race betrays no acquaintance with the fauna and flora of Asia. It may be replied that the fauna of Asia, excluding the "Indian region" of Wallace, differs little from that of Europe, and that anciently the difference was probably less than at present.

The remainder of the work is devoted to a description in detail of the various races and peoples.

We can decidedly recommend this treatise to all students who wish to acquire a comprehensive knowledge of ethnology as now understood, without being compelled to wade through a mass of learned journals and transactions.

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*Leçons sur lo Génération des Vertébrés.** Par G. BALBIANA, Professeur au College de France. Paris: Octave Doin.

THESE lectures, collected by Dr. Henneguy, form part of the course of comparative embryogeny delivered at the College of France during the winter term of 1877-78, and embody both a general survey of the present state of science in this direction and the results of the author's own researches. To give within the available space a full analysis of a work so abounding in details is of course impossible, but we shall endeavour to select the more salient points.

Proceeding from the theory of Knox, subsequently demonstrated by the researches of Waldeier, that every embryo is at first bisexual, he succeeds in throwing a welcome light upon the obscure phenomenon of parthenogenesis. He shows that, *e.g.*, in the Aphides, the ovum or female element is fecundated by the action of an embryogenous vesicule, which is the homologue of a spermatoblast of the male sexual gland, and from this fecundation there results the development of the egg up to the formation of a perfect animal. Hence, a female *Aphis* is histologically, though not morphologically, a hermaphrodite. Judging from the frequence of parthenogenesis among other invertebrate animals, a similar hermaphroditism is far from exceptional. In the silkworm a certain number of non-fecundated eggs undergo their normal development. Barthelemy records that on one occasion all the eggs laid by a virgin female proved prolific. Among other Lepidoptera it is certain that there is only a small number of males.† Among the Psychidæ parthenogenesis is an

* Lessons on the Generation of the Vertebrata.

† We are here on debateable ground; many authorities declare that among butterflies the males predominate in number.

ordinary fact. Among the Hymenoptera numerous species of *Cynips* have, according to the author, no known males. Dzierzon has detected parthenogenesis in the hive-bee. Even among vertebrate animals unfecundated *ova* undergo a partial development. The segmentation of such *ova* was observed by Bischoff in the frog, the bitch, and the sow, and these observations have been verified by more recent authors. Agassiz and Burnett recognised evident traces of segmentation in certain American species of *Gadus*. In the common hen, Oellacher discovered that the non-fecundated eggs experience a beginning of segmentation in the oviduct. But there is no known instance among vertebrate animals of the development having proceeded to the formation of a perfect animal.

It must be admitted, in the meantime, that this explanation of parthenogenesis raises a number of further questions. Thus the last generation of *Aphides* produced every season is composed of individuals of both sexes. Yet, in a heated conservatory, *Aphis rosæ* has continued to reproduce itself by parthenogenesis for four years in succession. Why is this? Why, among these insects, are the individuals produced by parthenogenesis brought into the world living, whilst those normally generated appear as eggs?

Concerning the spermatozooids, Professor Balbiani does not unhesitatingly admit the modern theory, which regards them as mere histological elements, analogous to the blood-corpuscules. He considers that there is something to be said for the old doctrine of their vitality. He holds that their movements display a certain degree of purpose, being directed to a definite end. Thus in *Bombyx mori* at the moment of intercourse the seminal fluid of the male is received in a special vesicle, which the next day is found completely empty, all the spermatozooids having emigrated into another capsule, which opens into the oviduct opposite to the former capsule, and where they await the *ova*, to fecundate them in passing. But the walls of the former receptacle have no contractile elements, so that the passage of the spermatozooids from one capsule to another can only be due to a spontaneous inward movement. In the former receptacle there remain only a few misformed and motionless spermatozooids. There exists consequently a kind of "natural selection," even among the sexual elements.

On the Nature of Life: An Introductory Chapter to Pathology. By Ralph Richardson, M.A. Second Edition. London: H. K. Lewis.

We have here an exposition and a criticism of the three prevalent theories of life, the so-called physical, the vital, and the

natural. The first, with which the name of Prof. Huxley is especially connected, views life as "a mere form of energy," analogous in its operation and relations to heat, light, magnetism, or electricity. At the same time it is regarded as "a property of the protoplasm, similarly as we ascribe ordinary physical properties to inorganic and inert substances."

On the other hand, the "vital theory," as advocated by Dr. Lionel Beale, regards "life as a power, force, or property of a special and peculiar kind," temporarily influencing matter and its ordinary forces, but entirely different from and in no way correlated with any other.

Finally, the "natural theory," advocated by John Brown in 1770, by Fletcher in 1826, and by the author in the present work, views life simply as "the sum of the actions of organised beings."

It is a characteristic and valuable feature of Dr. Richardson's work that he protests against that vagueness of language, which is especially dangerous in biological speculation. How often is the distinction between a property, a power, and an action forgotten! The author contends that "a property signifies only a susceptibility of motion; a power, only a means by which this susceptibility may be called into action, whilst an action designates the phenomena resulting from the two (power and susceptibility) in co-operation." Again, in his Harveian Oration for 1870, Sir W. Gull remarked—"The vegetable kingdom is no more than *an expression* in a higher form of the terrestrial conditions which even common experience proves to be in a general way necessary to vegetable life," and, further, the organisation of our bodies is "the expression of the highest correlation of these external conditions." Dr. Richardson asks for the precise meaning of this word "expression," and shows that in its mathematical sense it is quite inapplicable to individual objects. He then quotes Dr. Beale's arguments in opposition to Huxley's theory, none of which have been controverted. The phenomena of assimilation, secretion, and reproduction "differ absolutely from any actions known to occur in any kind of non-living matter whatever."

Nor is the vitalist theory, in turn, found more satisfactory. It is unsusceptible of demonstration. That life is required for the first formation of organised tissues cannot be denied; but it is the life or living action of the thing organising, *i.e.*, of the progenitor of such organism, and not of some Vital Force, Power, or Influence.

We think this work may be very profitably studied as an antidote to the modern diseases of vagueness in language, and of rashness and dogmatism in theory.

The Philosophy of War. By JAMES RAM. London: E. J. Davey.

THIS is a difficult book for a critic who, like ourselves, is at once by inclination and by duty barred from any excursion into the thorny fields of party politics. The author holds that the hand of God has been shown in times past in the chastisement of nations and their rulers, in the transfer of empire from one people to another, in the very substitution of race for race—all by war. He considers, therefore, that war is an ordained method of procedure on the part of Nature; that its sufferings and miseries are but a small item in the incessant sacrifice of life, whether human or brute, which is constantly going on; that individual happiness is not so much the aim of Providence as is the elevation of the type. He believes that man, like every other organism, is raised and ennobled only or mainly by the struggle for existence of which war is merely one of the more open manifestations. It is difficult to see how anyone who believes in the benefits of competition can come to a different conclusion. For in truth all competition is war. If A, by superior strength, bravery, activity, cunning, or practice in the use of arms, kills B outright, or reduces him to serfdom, the "friends of peace" express their horror; but they fail to see how exactly similar is the process when A, by dint of a larger capital, of greater skill and tact, perhaps even of a more unscrupulous disposition, undersells B, takes his business from him, and reduces him to poverty, perhaps to pauperism or to starvation. In the latter case the suffering is less acute, but more prolonged.

One argument in favour of the idea that war is specially sanctioned and ordained by Providence has escaped the author. The very arrangement of land and water on the surface of the globe is eminently favourable to aggression and invasion. Suppose, for instance, there had been a "silver streak of sea" between France and Germany, how the history of Europe, from the days of Louis XIV. downwards, would have been modified. Suppose that another channel of salt water joined the Adriatic to the Black Sea, cutting off the Balkan peninsula from North-eastern Europe, would there ever have been an "Eastern question" at all?

But we may go still further: unless geologists are utterly mistaken, the successive revolutions of the earth's surface have tended in the direction of opening roads for armies and closing the paths of commerce. Thus there is every reason to believe that in pre-historical ages the Red Sea joined the Mediterranean. Had this feature remained there would have been no Suez Canal requiring a watchful guardianship, and no possibility of Egypt being one day overrun from the north. We are told that a vast inland channel extended from the Black Sea to the Pacific

somewhere about the mouth of the Amur. Had this region not undergone elevation, so as to be converted into dry land, there would have been no need of any Afghan war.

But does the struggle for existence really lead to the elevation of individuals, of races, or of species? It is a remarkable fact that when man undertakes the improvement of any species his very first act is utterly to stamp out that competition from which certain theorists expect so much. He supplies the wants of each individual so fully that it does not require to expend any part of its energies in any struggle. We have seen this question happily illustrated by reference to a turnip-field. The farmer tolerates no competition either between individual turnips or between the latter and any interlopers in the shape of weeds. The consequence is, not merely a higher general average size among the roots, but the production of some individuals far surpassing any that can be developed in a competitive—*i.e.*, an over-crowded and unweeded—field.

Nor when a struggle takes place are we at all sure that the survivor, though possibly the one most in harmony with the surrounding circumstances, is by any means the best. What, for instance, are the plants which multiply and spread the most rapidly, and which, so far from needing our fostering care, bid defiance to all our efforts for their destruction? Is it the wheat, or the vine, or the rose, which thus obtrudes itself everywhere? One of the most obvious characteristics of rampant weeds is their generally low position in the scale alike of utility and of beauty.

In the animal kingdom we find something altogether similar. The commonest insects are, as a rule, the most mischievous and the ugliest. In proof we refer to the locusts, the phylloxera, the plant-lice and scale-insects, the various mosquitoes and sand-flies, the common house-fly, the wireworm beetles, the cockchafer, and many more. Now it is generally admitted by modern naturalists that a numerous species is one which is gaining ground in the struggle for existence, whilst a scarce species, on the contrary, is one which is verging upon extinction.

The same rule prevails among birds. The commonest species in Europe, the sparrow, is precisely the one for which least can be said.

After extending, as we might, this survey through other departments of animated nature, we are surely entitled to ask if the struggles between different human races and nations are quite as ennobling and elevating as our author believes, and whether war is quite so sure to end in the triumph of the best?

Whilst unable to refrain from the expression of these doubts, we hold that Mr. Ram's treatise contains many valuable lessons for a nation like ourselves, in whom the "tribal instinct" and the very impulse of self-preservation, if not decaying, are being systematically attacked and denounced as criminal and selfish.

On Phthisis and the Supposed Influence of Climate. Being an Analysis of Statistics of Consumption in this part of Australia. By WILLIAM THOMSON, F.R.C.S., F.L.S. Melbourne: Stillwell and Co.

CLIMATIC influence upon phthisis and tuberculosis is a question which is daily growing in importance in the scientific world. So much of the mysterious is still involved in the pathology of the disease that any straw is eagerly clutched at, and too many are willing to attribut to fog and mist attributes to which they have no just claim. Phthisis is admittedly hereditary; is popularly supposed to be influenced by climate; and generally understood to be non-contagious, and amenable only to prophylactic and palliative treatment. Are these surmises correct? That it is hereditary may be taken for granted. Is it non-contagious? If so, why is it that the apex of the lung is most liable to be attacked? Is it possible that true phthisis arises in the first instance from inhalation of living organisms that propagate in the tissues? If so, does not the root of the etiology of phthisis lie rather in sanitation than in climatic influence? And, further, if this be granted, is it not rational to suppose that direct contagion may take place? That direct contagion is possible is tacitly admitted, if not openly confessed, by the frequent counsel given by the profession to patients of a scrofulous tendency to remove to open spaces in hilly districts where the climate is equable, dry, and bracing. Here, avoidance of over-crowding is the clearest benefit obtained, climate having but little apparent influence. In the case of Switzerland, for instance, Prof. Parkes says—"Although on the Alps phthisis is arrested in strangers, in many places the Swiss women on the lower heights suffer greatly from it: the cause is a social one; the women employed in making embroidery congregate all day in small, ill-ventilated, low rooms, where they are often obliged to be in a constrained position; their food is poor in quality. Scrofula is very common. The men who live an open-air life are exempt."—(*Practical Hygiene*, 1878, p. 440.)

The remarks thus applied to Switzerland are apparently equally applicable to Victoria, and the colony of New South Wales generally. Hitherto it has been *de rigueur* to prescribe emigration to this colony as a prophylactic of phthisis; but a recently published work of Mr. William Thomson, F.R.C.S., of South Tana, goes far to dispel this illusion. In this exhaustive statistical treatise Mr. Thomson shows that during the years 1876, 1877, and 1878, 3003 persons died of phthisis in the colony of Victoria, of whom 762, or 25·37 per cent, were born in Australia; 2003, or 66·70 per cent, had resided there upwards of five years; 112, or 3·73 per cent, had lived there upwards of two years; and

only 126, or about 4 per cent, had lived there less than two years.

The first assumption of theorists upon reading, in the "Victorian Year-Book for 1878," that phthisis had "resumed its place at the head of the list of causes of death," was that the influx of persons more or less affected with tubercular disease would account for the increase; but the figures of Dr. Thomson prove that this surmise is incorrect. What deduction are we to draw from this? The climate of Victoria is, theoretically, specially adapted for such as suffer from pulmonary complaints: neither the open spaces nor Eucalyptus trees are wanting, and the death rate in town is not strikingly greater than in the country. Thus during the year 1878, of all deaths from phthisis in Victoria, only thirty-six more occurred in town than in the country. From this it would seem that the influence of climate has been considerably over-rated when dealing with the etiology of phthisical disorders, and from hereditary transmission and non-contagion attention must be turned to generation and direct contamination. In this research the worker may be aided by the recognised value of sea voyages, or the possible influence of bromine and iodine upon persons of scrofulous or consumptive tendency. By analogy it might be argued from this that the minute organisms giving rise to phthisis are parasitic, since bromine and iodine are most destructive to parasitic life of every form.

The argument thus resolves itself into the question, Is phthisis a parasitical disorder? If so, spontaneous generation, within the control of sanitation, and direct contagion follow as inevitable corollaries. Whilst the pathology of the disorder remains as at present, so obscure, any indication of direction in research will be welcomed by scientific investigators.

A Monograph of the Silurian Fossils of the Girvan District in Ayrshire. With Special Reference to those contained in the "Gray Collection." Fasc. II. By H. Alleyne Nicholson, M.D., D.Sc., &c., and R. Etheridge, Jun., F.R.G.S. Edinburgh and London: W. Blackwood and Sons.

We have here a continuation of the valuable monograph which we have previously had the pleasure of noticing. The present part is entirely occupied with the Crustaceans from the Silurian rocks of Girvan, and treats of the Trilobita, the Phyllopoda, the Cirripedia, and the Ostracoda. It is illustrated with six plates, and contains a table of the geographical distribution of the crustaceans in the Girvan district.

Memoirs of the Science Department, University of Tokio, Japan. Vol. I., Part 1. By E. S. MORSE. Tokio: published by the University.

THIS volume is devoted to an examination of the shell-mounds of Omori. These mounds, in addition to shells, contain bones of monkeys, deer, boars, wolves, and dogs, and give plain evidence of the former prevalence of cannibalism.

The author has also detected the jaw of a large baboon-like ape, possibly a Cynopithicus, which now occurs in the Philippines. Japanese tradition tells of monstrous apes formerly found in the islands.

We may mention that the paper, the letter-press, and the illustrations have all been produced by native labour.

Lecture Notes on Physics. By CHARLES BIRD, B.A., F.R.A.S., Second Master in the Bradford Grammar-School. London: Simpkin, Marshall, and Co. Bradford: T. Brear.

THIS little book is essentially examinational. More than one-third of its bulk consists of a list of the "questions set in the Science and Art Examinations" from the years 1867 to 1879 inclusive. Thus both those who are performing and those who are undergoing the operation known in polite circles as "preparing for an examination"—but described by horrid, plain-spoken people in a word of one syllable—may form a tolerably accurate notion of what they may expect. It must not for a moment be supposed that we attach any blame to Mr. Bird. He is merely accommodating himself to existing circumstances. Unless he acts thus his pupils might possibly "know," but they would fail to "pass," and he himself would suffer in reputation accordingly. He therefore does what we must all do more or less until we become enlightened enough to make a clean sweep of the whole system. Will this be done before it has quite eaten out the intellectual life of the nation?

The notes are clear, intelligible, and accurate, and, if properly filled up with details and illustrated with the necessary experiments, will form a good course on physics.

CORRESPONDENCE.

THE FERTILITY OF HYBRIDS.

To the Editor of The Journal of Science.

Sir,—In a contemporary of yours a correspondent ventures upon the bold statement that "of the 20,000 species of animals" no two species have been found to produce fertile hybrids. Passing over the facts that the known species of animals approach 200,000 much more nearly than 20,000, and that not in one case out of one hundred has the fertility or non-fertility of hybrids been fairly studied, I would refer this writer to Mr. J. A. Allen's "History of the American Bison," published in the "Ninth Annual Report of the United States Geological and Geographical Survey." On page 585 he will read that the "buffalo" (bison) interbreeds freely with the domestic cow, and that the half-breeds are fertile.—I am, &c.,

Truth-Seeker.

BAMBOOS.

To the Editor of the Journal of Science.

Sir,—In Wallace's "Tropical Nature" (p. 53) we read as follows:—"The gigantic grasses called bamboos can hardly be classed as typical plants of the tropical zone, *because they appear to be absent from the entire African continent.*" This assertion, when the name of Mr. Wallace is connected with it, appears most extraordinary.

On the West coast of Africa, and far in the interior, the huts of the natives are composed of split bamboos. The dwellings of those in the British Settlements certainly are. In a journey I made into the interior I found both cane and clay dwellings; the fences of the private enclosure were almost invariably, I may say, of interlaced split cane. In the forests I met not only with

hollow-jointed bamboos, with the peculiar glazed flint surface, but long trailing canes interlacing the forests, of many—perhaps more than a hundred—feet long. How long I do not know; they seemed interminable. In one place I came to a cane brake said to be 16 miles long, through which passed the road, and which, for the mile or two I rode in it, was interlaced overhead by the canes—a species of natural arbour. If I am not mistaken De Chaillu speaks of the canes interlacing the forests he visited. Either Mr. Wallace must be in error or the flinty grasses I saw were not bamboos, yet they exactly agree with the description contained in Baird's "Student's Natural History."

I have no doubt Mr. Wallace is most accurate in his descriptions of Malayan and South American tropical products, but he seems to have fallen into an error in respect to Tropical Africa. And this is the more curious if there be any reality in his half-uttered hypothesis (Malay Archipelago), that Celebes was once connected with the African continent.—I am, &c.,

S. B.

EDMUND HALLEY.

"In the 'Journal of Science' (formerly called the 'Quarterly Journal of Science,' but now, having become monthly, styled as above) for this month we were surprised to read, in an article on 'Edmund Halley, his Life and Work,' that 'in 1720 he was appointed Astronomer-Royal at Greenwich, in room of his old friend and colleague Flamsteed.' Irony would indeed be out of place on such a subject, but we trust that the biography of the second Astronomer-Royal, which Prof. Pritchard is understood to be preparing, will throw some additional light upon the unfortunate and ever-to-be-regretted disputes between him and Flamsteed, which must always form a sad episode in the history of astronomy, leading as they did to feelings, on one side at least, the reverse of friendly."—(*Athenæum*, No. 2729, Feb. 14, 1880.)

To the Editor of the Journal of Science.

Sir,—The above paragraph in the "Athenæum" has been brought to my notice, and as the writer of the article in question I would take some notice of it.

As you are aware, I did not pretend to give anything like an exhaustive monograph of Edmund Halley. The sketchy article

was only intended to help to draw public attention to the proposed erection of a monument to Dr. Halley in St. Helena, which proposition was first mooted in the pages of "Nature," on January 29th, 1880. Being absent from London, and unable to consult the "Philosophical Transactions," &c., I expressly asked assistance from Prof. Pritchard, the Savilian Professor in whose hands the Halleian papers have been since 1875, *i.e.*, over four years since. I was emboldened to do this from the great courtesy I had experienced from other astronomers, who heartily have approved of my friend Mr. Gill's scheme; but the Professor of Astronomy at Oxford did not answer my letter.

I judged that a former and early friendship had existed between Flamsteed and Halley, from the latter having been employed to settle an *amicable* discussion between the second Astronomer-Royal, or rather King's Astronomer, and Cassini, according to the account given in the "Encyclopædia Britannica" (ed. 1828).

I shall be only too happy to have more mistakes and errors pointed out in my account of Halley, as the more notice attracted to the memory of the great man the better, and my intended object will have been accomplished. The Astronomer-Royal has signified his approval of the scheme, which will shortly be carried out.—I am, &c.,

THE WRITER OF THE ARTICLE.

PS. Anyhow attention has now been drawn to the inexcusably long time during which Prof. Rigaud's materials have been delayed from publication by the Rev. Chas. Pritchard. The public have been very patient, but the scientific world has a right to demand the fulfilment of the expectations held out in 1875.

NOTES.

Mr. E. A. Thompson writes to the "American Naturalist" that certain moths, *Plusia precationis*, having been caught by their tongues in the pollen-pockets of *Physianthus albens*, an Asclepiad plant, were stung to death and devoured by what were supposed to be ordinary honey-bees. Dr. Hermann Müller considers the fact of the moths being thus entrapped new and interesting; but mentions that his brother, Fritz Müller, in South Brazil, has observed bees eagerly licking the juice dropping from pieces of flesh which had been suspended to dry in the air. Mr. Darwin suggests that the bees may possibly tear open the bodies of the moths in order to get at the nectar contained in their stomachs. Both these distinguished naturalists recommend further observation. It is stated by Prof. A. J. Cook, of the Michigan Agricultural College, that bees kill the drones not by stinging, but by tearing with the mandibles.

According to Prof. Church, withered leaves of the usual autumnal colours—yellow, red, or brown—can be rendered green again by steeping in water along with a little zinc-powder.

Dr. Auerbach, writing to the "Chemiker-Zeitung," mentions as a curious fact that during an entire summer he observed water-beetles—probably *Gyrinus natator*—living in tanks of a saturated solution of Glauber's salt. When alarmed the beetles took shelter under the crystals, just as they do in ordinary circumstances under water-plants. A little of the liquid so harmless to insects, having found its way by leakage into an adjoining river, proved fatal to multitudes of fish.

Dr. Yandell, in a letter to the "Louisville Medical News," speaks of a fertile female mule, now to be seen at the Jardin d'Acclimatation, Paris. She has brought forth no fewer than six foals—some by zebras, some by an ass, and some by a stallion.

Dr. Erlenmayer, in the "Medical and Surgical Reporter," gives it as his opinion that the Semitic nations, including the ancient Hebrews, were left-handed, and that this peculiarity was the reason why they wrote from right to left. He adduces evidence in favour of his theory both from the Talmud and the Old Testament. We must, however, remember that in one of the earlier books of the latter (Judges, iii., 15) left-handedness is mentioned as a personal peculiarity.

The chloride of methyl is now used in extracting the odoriferous principles of plants. It enables the manufacturer to dispense with the tedious process of *enfleurage*.

Dr. S. Schneck and Dr. Elliott Coues communicate to the "American Naturalist" decisive instances of the mischief done by the European sparrow, which has been rashly acclimatised in America, and of its utter failure as a remedy for the cabbage caterpillar (*Pieris rapæ*).

W. H. Ballon communicates to the "American Naturalist," on what appears to be good authority, a case of a considerable number of swallows being found torpid in the hollow of a tree, in April.

According to Dr. Staub's observations, published in the "Botanische Zeitung," the flowering period of trees is hastened only when the mean temperature for the month is at least 3·5° F. higher than the average: a smaller increase does not affect vegetation. On the other hand, the smallest fall in the temperature of the month occasions a retardation.

Papilio alexanor, a very local species of butterfly from the South of France, is considered by many entomologists to be a hybrid between *P. machaon* and *P. podalirius*.

Prof. W. J. Beal considers that, in countries where they occur, humming birds must play a very important part in the fertilisation of flowers.

According to Dr. Jousset de Bellesme, cocoons do not serve insects as a protection against the cold. The pupa resists congelation by reason of a continuous liberation of heat due to the destruction of the muscular system of the larva, which is much more considerable than that of the mature insect. The large quantity of uric acid discharged at the time of metamorphosis is a proof of the extent of the organic transformations which have taken place.

From observations on the cattle of Brittany, M. Bellamy is led to question the common doctrine that in-breeding is injurious.